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PHILOSOPHICAL ESSAYS.

BY

HENRY EELES, Esq;

CONTAINING

His Discovery of the Cause of
THUNDER, with a subse-
quent Explanation and De-
monstration of the same.

The Cause of the Ascent, Sta-
tion, and Descent of VA-
POURS and EXHALATI-
ONS: The Cause of
WINDS, and an Explana-
tion of the general Pha-
nomena of the WEATHER,
BAROMETER, &c.

The true Rudiments and The-
ory of ELECTRICITY and
MAGNETISM.

An Attempt to shew that the
ELECTRICAL POWERS
are the Cause of the
REFLECTION and RE-
FRACTION of LIGHT.

Some Mention of the won-
derful MEDICINAL EF-
FECTS of ELECTRICITY
on a great Number of Pa-
tients, whom he has cured
of Palsies, and many other
Disorders. With other
useful Matters, which may
be deduced from these
ESSAYS.

D U B L I N :

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in CASTLE-STREET. 1771.

PHOTOGRAPHIC

ESSAYS

THE LIFE OF



U. M. I.

P R E F A C E

T O T H E

L E T T E R S.

NEWTON in his immortal researches discovered, by effects which were sensible to him, that some subtile medium, of great elastic force, must necessarily supervade the surfaces of all bodies, to cause the reflection and refraction of light, &c. but being professedly ignorant of what that medium or power may be, he said that he would call it æther.

From the modern discoveries about electricity, many gentlemen have declared that electricity was the æther of Sir *Isaac Newton* ; but being ignorant of the mediums or powers which constitute what we call electricity, and consequently ignorant of the nature and properties or general laws by which those powers act, it was impossible for them to shew how electricity was adapted to the purposes, to which that great man applied his æther.

For hitherto electricity (except what may be found in the following tracts, and what I shall mention) has been considered as a subtile medium subsisting they knew not where, and acting they knew not how. I now find that some gentlemen, who first enquired into this matter, supposed that there were two kinds of electricity; and some others followed their opinion; particularly Mr. *Muschenbrock* has asserted that there were two kinds of electricity, and that any body may be endued with either kind at pleasure; but that it was impossible that any body should be in possession of both at once; but where that gentleman has been mistaken, the following tracts will shew; and that it was impossible for him to have explained, his own famed and very useful experiment of charging a bottle, by that doctrine.

Mr. *Benjamin Franklin*, who has been greatly followed, especially by the English, has produced another doctrine of electricity. He supposes that electricity subsists in glass, and that glass by being rubbed, throws out that power, and that sealing-wax, resins, &c. drink it in. And in electrifying other bodies, he says that some may be electrified positively and some negatively; which are terms I must confess that I do not understand; but what I think he must mean by them is, that when a body is positively electrified, there must be part of that power which is thrown out from the glass, stopped or retained in that

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that body; and that must be the cause of that body's repelling another body for electrified; and when a body is negatively electrified, it must be divested of part of its natural share of electricity; but if electricity subsists only in glass, and not in all other bodies, how is it possible to divest a body of what it never had? and he says that two bodies negatively electrified, repel each other as much as when positively electrified. But to me it appears a perfect contradiction to say that two bodies shall repel each other the farther, the more they are divested of the repulsive power. This plain contradiction, one would think, might have made this ingenious gentleman suspect that there was something fallible or deficient in his doctrine of positive and negative electricity; yet in some experiments communicated to the Royal Society, he makes it a principle in his doctrine of electricity; where he has mentioned three under the express title of principles, in the 49th Volume of Philos. Transf. page 300.

His first principle. Electric atmospheres that flow round non-electric bodies, being brought near each other, do not readily mix and unite into one atmosphere, but remain separate, and repel each other.

Second principle. An electric atmosphere not only repels another electric atmosphere, but will also repel the electric matter contained in the substance of a body approach-

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ing it : And without joining or mixing with it, force it to other parts of the body that contained it.

Third principle. Bodies electrified negatively, or deprived of their natural quantity of electricity, repel each other, (or at least appear to do so, by a mutual receding as well as those electrified positively, or which have electric atmospheres.

His first principle is true, when the atmospheres are of one kind; but quite false when they are of different kinds; for there they attract each other and instantly join; as will be seen in the following, and all other experiments.

In his second principle he allows that the electric matter exists in all bodies; tho' in several of his writings he confines it to glass. But before we can know how an electric atmosphere acts on this electric matter, we must enquire what this electric matter is, and of what it consists, and how it acts and is acted on. If it consisted of a single medium or power, his doctrine would in some measure be true, but the effects which always appear would not follow. But this electric matter, or what we call electricity, consists of two different distinct elastic mediums or powers, which equally and strongly attract and condense each other, and are equally attracted by all matter, according to the specific gravity of the matter, as will appear in the following tracts. Therefore when any body is immersed in an electric atmosphere, that atmosphere only repels that power of electricity

electricity which is of the same kind in the body; but equally attracts the power of a different kind which is in the body; and while that body remains so immersed, the different powers in that body remain separate with their different atmospheres at each end of that body; which will be more particularly explained in what I call an *experimentum Crucis*, and many other experiments in the following tracts.

As to his third principle, it is quite erroneous to suppose that bodies by being negatively electrified (as he terms it) are deprived of their natural share of electricity: for they are no more deprived of that, than when they are positively electrified; and I am surprised to find that a gentleman, so conversant with electricity, should think that bodies electrified negatively, are without electric atmospheres; when all experiments shew that they have as extensive atmospheres as when electrified positively; and that they act in all respects in the same manner. For when a body is electrified negatively, it is impossible for him or any one else to tell, by any effects of that body on another body which is not electrified, by which power that body is electrified; nor is there any way of discovering it, but by bringing another body electrified with a known power, to try whether its atmosphere attracts or repels the atmosphere of the body electrified with an unknown power.

Now it is plain from these principles, and by all the other writings of this gentleman,

which I have seen, that he makes electricity consist of one single medium or power; without shewing how this power may be found with an extensive atmosphere, or how that atmosphere can produce the phenomena which arise from electrical experiments. We are told that friction makes glass throw it out, and that sealing-wax, resins, sulphur, &c. drink it in. But how is this done? This supposition is very fallible; for I can make glass throw out the resinous or vitreous power at pleasure, or I can make sealing-wax throw out the vitreous or resinous power as often as I think fit, and I can alter this action of the wax twenty times in a minute. I call the powers resinous and vitreous for distinction sake; for I do not know any essential difference to distinguish them by.

But supposing there was but one kind of electric atmosphere; by what action can it divest a body of its natural share of the electric matter? since this gentleman allows that the electric matter exists in all bodies. It is intelligible how it may add to it, but not how it can take from it. In his explaining the charging and discharging the *Leyden* bottle, he is obliged to have recourse to a strange hypothesis; that there is some stratum or partition in the middle of glass which separates the two sides; and that when the electric fire is accumulated on one side of the glass, the other side parts with as much of its natural share, as was received by the first;

first; and that when a communication is made between the sides, by any conductor of electricity, the emptied side greedily drinks up the electric matter accumulated on the other side. But this is an entire mistake; for when the bottle is charged, it is equally electrified on both sides, but with the different powers of electricity; and when a communication is made by a conductor, the increased power without, flies in; and the increased power within flies out; to make the powers equal to each other within and without; for these powers strongly attract and condense each other when equal to each other; which is their natural state in all bodies; and therefore they do not exert any sensible action in that state. Glass may be equally electrified on both sides with either the resinous or vitreous power; and this may be done by excited glass or wax when they throw out the usual powers; or it may be done by the atmosphere of any body electrified by glass or wax; or the *Leyden* bottle, suspended by silk, may be charged with either of the powers inside and the other power outside, by only immersing it in the atmosphere of a body electrified with either of the powers; and may be discharged, and recharged in the different order, without contact or communication with any thing but that atmosphere. These experiments, or any other, cannot be explained by the action of one single medium or power. From this imaginary stratum or partition in glass, this gentleman

gentleman concludes that glass is impervious to the electric powers ; but in this he is also mistaken ; for with excited wax or glass, I can electrify a body through glass, with which ever power a spectator shall please to name at the instant of performance. I have shewn these, and many other experiments to a great number of gentlemen, some of them conversant with electricity, who thought them sufficient to demonstrate to the meanest capacity, that two different distinct mediums or powers did exist in electricity ; and that they acted by the laws which I have mentioned in my letters. But as the design of these letters was to shew the existence of these powers, and by experiments to demonstrate the general laws by which they acted ; I shall refer to them.

The letters, which contain the doctrine of these different powers in electricity, were sent to the Royal Society, on the 9th of August, 1757, and 14th of February, 1758.

In the end of the year 1759, in the vol. Transactions, page 371, *Robert Symmer, esq;* has produced a doctrine ; that electricity does not consist of one positive power, as generally supposed ; but of two distinct, positive and active powers ; which, by contrasting, and, as it were, counter-acting each other, produce the various phænomena of electricity. I here use his own expressions.

At first I really thought that this gentleman might have taken hints from my letters ; but,

but, on more mature consideration, I think his own experiments might have led him to this doctrine; in which, as far as he has gone, he is certainly right; but he went but a very little way to explain this matter. But, as Mr. *Symmer* says, that Mr. *Franklin* could not be brought to agree with him; I suppose is the reason that Mr. *Symmer* has not since pursued his discovery. I have tried his experiment with black and white silk stockings; and find it true, that the black stocking is always electrified with the same power, whether it is put inside or outside; and the white stocking always electrified with the contrary power. This experiment is a very extraordinary one; and, I think, may possibly lead to a discovery how the clouds come to be electrified with the different powers of electricity; which is the great desideratum, to demonstrate the whole actions of the clouds, in their ascent and descent, and relative to thunder, rain, &c. But the want of this knowledge does not any way invalidate, or retract from what I have said on this subject; since the clouds are found to be really so electrified.

In the year 1767, Mr. *Joseph Priestley* published his history of electricity; in which he attempts to mend Mr. *Symmer's* theory, by a supposed theory of his own. But he so regularly supposed the theory which I gave in the following tracts, (long before Mr. *Symmer* or he thought of it) and used my terms and expressions, that I cannot forbear thinking

ing that he had better have brought my experiments to prove it, (since he produced none of his own) and put my name to it, than to have left it a suppositious theory of his own. Whether, in this, he is, or is not a plain plagiarist, I must submit to the readers of both.

It will plainly appear, that I have not supposed the existence of these different powers; but that I have demonstrated by the effects of electrical experiments that they do really exist; and have also thereby shewn the general laws by which they act, and are acted on. And, if more experiments are wanted, I could give a thousand; nor did I ever meet with any experiment which did in the least contradict what I have said of these powers, or their manner of acting; though, for many years past, I have applied them to various purposes; particularly to medicinal use; by which I have cured above five hundred patients of different disorders, without applying any other medicine internally or externally. Among which were many paralytics; some with hemiplegias, some with paraplegias, some with palseys, that began in the extremities, and worked up beyond the waist, so that they knew not when their excrements went from them. I have cured rheumatisms (not of the inflammatory kind) almost instantaneously, in a miraculous manner; agues, jaundice, obstructions in women, and many other disorders, in which I thought the effects of these powers might do service. Several

veral physicians have submitted to be my patients, when the known materia medica, after long trial, has not answered their purpose. I offered to send an account of these cures, and the manner of performing, to the Royal Society; but, as they did not think proper to answer my letter, I did not think it worth the while to trouble myself with so prolix a detail as was necessary to explain this affair; and as to their not answering, or publishing many of my letters, I think that I have great reason to complain; as I shall shew presently.

But, to return to my friend, Mr. *Priestley*, who has so methodically supposed my theory of electricity. Though he is so good at supposing, can he possibly suppose that any man will think that he could suppose so regular a theory of electricity, which so easily accounts for all the phænomena thereof, and which has puzzled all the ingenious men of the world to discover, without his producing one single experiment to shew the existence of the different powers, or their manner of acting; or, when any man has read these letters, which were addressed to the Royal Society, of which Mr. *Priestley* is a Fellow, must he not think that Mr. *Priestley* has copied his theory from them? as, indeed, he has followed them very close in his suppositions; for he only supposes the actions of these powers all along; but he has neglected to suppose the principal action, which I have mentioned, and which removes all difficulty
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in explaining all their other actions, *viz.* That, when they are equal to each other, they not only attract each other, but condense each other into almost an insensible compass; which is their general state in all bodies, as they are both equally attracted by all matter; and that when they are any way separated, which must be done by encreasing the one and lessening the other power, for they never can be entirely separated in any body, the encreased power by its elastic force will expand itself through a space many millions of times greater than what it took up in its condensed state; as I have often proved by experiments subject to sense.

Now, if any gentleman can think this hypothesis is Mr. *Priestley's* own, he must, at least, do me the honour to say, I have been ten years before him; and, if real experiments are better proofs of the truth of such a doctrine, than bare suppositions, he must also allow that I have proved it better and clearer than Mr. *Priestley* has done. Had the Royal Society thought fit to publish these letters, it would have saved Mr. *Priestley* the trouble of so much supposing; but I must take the liberty of supposing that Mr. *Priestley* thought I was dead, and that these letters would never come to light.

Before I speak of my design in publishing these letters, I will shew that the doctrine given by Mr. *Franklin* and others, is fallible and insufficient.

First,

First, Mr. *Franklin* supposes that glass is impervious to the electric powers. This may be confuted by so easy experiments, that I am surprized that Mr. *Franklin* could have missed them. Take a piece of bog-down, suspend it by silk; then take a pane of clean sash-glass, and warm it, and let the down hang by the side of it; then bring an excited electric to the other side of the glass, and the down shall fly off perfectly electrified, in the same manner as it would have been if the glass had not intervened. If the excited electric be glass, the down will be electrified with the vitreous power; and if wax, with the resinous power. This shews that the powers pass entire through the glass; but if Mr. *Franklin* chooses to think that they are different powers, thrown from the opposite side of the glass, he must then allow that two different distinct powers do exist in all glass; which oversets his whole doctrine, which depends upon the action of one single power. In the next place, Mr. *Franklin* says that glass cannot receive electricity at one side, without parting with so much of its natural share at the other side; and on this supposition depends his doctrine of charging the *Leyden* bottle. But in this he is entirely mistaken; for you cannot electrify a pane of glass at one side, but the other side will be equally electrified with the same power, whether resinous or vitreous; except you form a communication from one side of the glass with some non-electrics, while

while you electrify the other side ; and then, be your excited electric either resinous or vitreous, it will repel the power of the same kind from the opposite side of the glass, and attract the different power from the non-electrics ; and these powers will hold each other affixed to the glass, by their natural attraction through the glass ; and thus the glass may be electrified with the different powers at each side ; and thus the *Leyden* bottle becomes charged ; for if you set your bottle upon a clean electric stand, so that a different power from that you attempt to charge it with cannot be drawn up to the outside, you'll find that both sides are electrified with the same power whether vitreous or resinous.

But, to electrify a pane of glass equally at both sides, either with the resinous or vitreous power, and that by the atmosphere of a conductor electrified by either of the powers. Pass ten strong knitting needles through a stick ; so that they may lie parallel to each other, (and if they are pointed it will do best) and within less than half an inch of each other ; then fix a wax or glass handle to the stick ; when your conductor is electrified, and the wheel turning, bring your pane of glass within three inches of the conductor, and draw the points of your wires which are next to the conductor three or four times slowly over the glass, beginning at the side of the glass next to the conductor, and drawing them from the conductor, and you shall find that glass equally electrified at both sides,

sides, with the power contrary to that which the conductor throws out: Or, if you would have the glass electrified with the same power, which the conductor throws out, place one end of the wires near the conductor, and draw your glass under or over the other points of the wires from the conductor, but in contact with the wires, and the glass will be electrified equally at both sides with the same power with the conductor. Or a pane of glass may be electrified equally at both sides with the vitreous power, by a large stick of sealing-wax excited; or equally at both sides with the resinous power, by an excited glass tube, in this manner. When your pane of glass is clean, dry, and warm, place a small bundle of linen rags, suspended by silk, or a sheet or two of paper folded like a letter, or any non-electric, upon the glass, and bring an excited glass tube under the pane, and in contact with it, or near approach to it, and when it has remained there two or three seconds, toss off the rags or paper, and instantly withdraw the tube; you will find the pane of glass electrified at both sides with the resinous power: and the same operation with a large stick of sealing-wax excited will leave the pane of glass electrified at both sides with the vitreous power; and the non-electric which was on the pane, electrified with a different power. Now to explain this matter; The two different powers of electricity being equally adhesive to the pane of glass (as indeed they are to all bodies) they attract and condense each other into almost

an insensible compass, and in that state do not exert any sensible action, but the atmosphere of the excited electric drives part of the same power out of the glass into the non-electric, and at the same time attracts the different power of electricity from the non-electric into the glass; now the non-electric and the excited electric being removed, the different powers in the glass are rendered unequal to each other, and therefore the encreased power by its elastic force, when the attraction of the other power is lessened, expands itself into an extensive atmosphere; which atmosphere will act in all respects like the atmosphere of any electric excited by the same power; for all electrics are excited in the same manner by the separation of these powers, which are always equally inherent in them and all other bodies till separated by friction, &c. And the reason that non-electrics cannot be electrified without being insulated by electrics, is because these different powers so quickly change place through non-electrics: for the atmosphere of an excited electric always attracts one power in the non-electric, and repels the different power in the non-electric; yet when the non-electric is removed from that atmosphere, the different powers in the non electric instantly rejoin by their natural attractions of each other, and become equal to each other, in which state they never exert any sensible action. But take an iron rod of a foot long with the ends rounded off, and suspend it by silk, and bring it within
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three or four inches of the conductor, while the wheel is turning; then take it out of the atmosphere of the conductor, and you will not find any sign of its being electrified; then bring it into the same position, and you will find the two ends electrified with the different powers of electricity; the end next the conductor with the power contrary to that which the conductor throws out; which may be known by touching the ends with a piece of bog-down suspended by silk. In the first case the iron did not receive any electricity from the conductor, nor could it receive any from the conductor in the second case, the circumstances being the same; therefore these different powers must be inherent in the iron, and must be thus separated by the atmosphere of the conductor, by attracting the one, and repelling the other; and in this separated state it is evident that each power appears in an electric atmosphere. And it is evident, that when the iron is taken from the atmosphere of the conductor, that these different powers do instantly reunite by their natural attraction, and are condensed so as to shew no signs of being electrified. But if this iron rod was sawed afunder at one third of its length from the conductor, and an hole drilled in each end to receive a peg, that it may be brought as one rod towards the conductor as before mentioned, and the pieces raken afunder while the ends were differently electrified, and carried from the atmosphere of the conductor; each piece will remain electrified with a different power;

er; which may be tried by the hog-down. But when these pieces come to touch each other, the powers are instantly and equally reunited through the whole, and all signs of electricity vanish.

The *Leyden* bottle suspended by silk, may be charged, and then discharged, and recharged with the powers in the contrary order, only by the atmosphere of an electrified conductor, in this manner; fix some sharp pointed wires to the coating of the bottles, so that they project an inch below the bottom of the bottle, and fix some sharp pointed needles to the rod from the inside of the bottle projecting upward; suspend the bottle by silk, and bring it that the rod from the inside may point to the conductor, while the wheel is turning, and let it remain there for some time; and you will find the inside of the bottle charged with the same power which the conductor throws out: then suspend the bottle over the conductor for the same time, with the wires from the bottom pointing to the conductor, and you will find the bottle discharged; but let it remain in that position as long again, and you will find the bottle recharged, but in the contrary order from what it was at first. The conductor being electrified with the vitreous power, the atmosphere of that attracts the resinous power from the inside of the bottle and from the non-electric contained therein, and itself enters in the room thereof; till the inside and non-electric therein are electrified like the conductor with the vitreous power;

er; which vitreous power within will repel the vitreous power from the outside of the bottle, and from the non-electric in contact with it, through the points of the wires, and attract the resinous power from the air through the points till it arrives at the outside of the bottle; where it meets a resistance from the glass, and therefore is accumulated there and held in firm contact by the attraction of the contrary power inside the bottle; which contrary power it equally attracts and holds affixed to the inside of the bottle: and thus the bottle remains charged when taken from the atmosphere of the conductor. When the experiment is reversed by bringing the wires from the bottom of the bottle pointed toward the conductor, the vitreous atmosphere of the conductor attracts the resinous power from the outside of the bottle and the non-electric coating, and adds to the vitreous power there; which vitreous power repels the vitreous power from the inside, and attracts the resinous power through the needles, till the bottle becomes charged in the contrary order from what it was at first.

There are many cases of this bottle, some of which I have mentioned in my letters, but they may be all explained (and indeed all other electrical experiments) by a due consideration of the actions of these two different powers, *viz.* That they are equally attracted by all matter, and they equally attract and condense each other into almost an insensible compass when equal to each other; but

when unequal to each other, the increased power by its elastic force expands into an electric atmosphere, which atmosphere will repel any power of the same kind, but equally attract any power of the different kind. But, as I have explained this matter more fully in my letters, I shall refer to them.

Except what I have said in the following letters, I do not find any one has attempted to shew how any electric comes to be excited or electrified; I mean to produce an electric atmosphere, which is very easily explained by the separation of these two powers, by lessening one and encreasing the other; as may be seen in electrifying the pane of glass at both sides with the vitreous power, by the excited wax; or at both sides with the resinous power, by the excited glass tube. In exciting electrics these powers are never entirely separated, but the lessened power acts inward to the electric, and the encreased power acts outward from the electric with an extensive atmosphere. For, when a large stick of sealing-wax is a little warm and well excited, I can electrify a piece of bog-down suspended by silk with the vitreous power, which acts inward to the wax; and again withdraw that power from the down, and leave it electrified with the resinous power, which acts outward from the electric, only by my manner of touching the down with the wax; and a spectator shall scarcely perceive any difference in the manner of touching the down with the wax.

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I could produce numbers of experiments, to shew the co-existence of these different powers in all bodies, which every careful experimenter must easily perceive; but I believe what I have said is enough to shew the insufficiency of attempting to explain all the different attractions and repulsions of electricity by one single medium or power. But no difficulty remains in explaining them all by the action of these different powers, when rightly understood.

Mr. *Franklin* has asserted that glass cannot receive electricity at one side, without parting with so much of its natural share at the other side. But this is demonstrably false, by the pane of glass being electrified equally at both sides with the vitreous power, by the excited wax, or with the resinous power, by the excited glass tube. Or, the *Leyden* bottle, set on an electric stand, will be found to be electrified at both sides with whatever power is thrown into it.

Mr. *Franklin* has asserted, that when the *Leyden* bottle is charged, the power, it acts with in discharging, is contained in the glass, and not in the non-electrics within and without the bottle; in which he is certainly mistaken; for the different powers in the non-electrics are as much separated in charging the bottle, as the powers in the glass; and, therefore, in their reuniting, on the discharging the bottle, they must add a force to the discharge, proportioned to the electric powers contained in them; and that

will be in proportion to their quantity of matter. Which may be easily proved by putting a thin coating inside and outside the bottle, and filling another bottle of the same size with quick-silver, or lead and water, and a coating outside of three or four folds of sheet lead; the different force of these bottles will be easily distinguished on the discharge. And this experiment I have tried; and here I may observe, that if Mr. *Franklin's* five eight-gallon jars had been filled with the most dense matter in the inside, and an equal quantity on the outside, I think that they would have deprived any man of his life.

I cannot forbear again expressing my surprise, that Mr. *Franklin* should imagine that bodies electrified negatively (as he terms it) should be without electric atmospheres, when all experiments shew the contrary. Electrify a piece of insulated bog-down, with the resinous power, or negatively, and you'll find all the fibres repelled in the same manner as they are when electrified with the vitreous power, or positively, as he calls it: Or, electrify the stream of a fountain, negatively, and you'll find it dispersed into a very great number of small particles, in the very same manner that it is when positively electrified. And from hence I made an artificial shower of rain, by electrifying the small streams of two fountains with the different powers of electricity, they were both dispersed into very minute particles, but by the

the attraction of the different powers, these particles ran together at top, and came down in heavy drops, like a heavy shower of rain.

Mr. *Nollet*, when he talked of an effluence and affluence, was so far right; but as he knew not that it was an effluence of one power, and an affluence of the other power, it was impossible for him to explain the phænomena of electricity by his effluence and affluence of the same power. That there is an effluence and an affluence, may be proved by many experiments; but I shall only mention one. In one of my letters, I have mentioned the dancing of paper puppets between my table and a dish suspended from the conductor: When you dry the head of one of these puppets, the power thrown out from the conductor cannot enter that puppet so freely, as the different power which is attracted from the table can enter at the feet, which are not so dry; and, therefore, there will be much more of the attracted power in the puppet, than of the power thrown out, and therefore this puppet will ascend to the dish, and remain there; but reverse this experiment by drying the feet and wetting the head, and the puppet will remain fixed to the table; because there is more of the repulsive power in the puppet than there is of the attracted power. But when there is retained in the puppet so much more of the attracted power, as will balance the gravity of the puppet, than there is of the different power proceeding from

from the conductor, the puppet will be suspended between the dish and the table. And this may be easily contrived in the shape of the puppet ; for the head, being round, does not admit the power coming out, so easily as the feet, being sharp, admit the power going in. This experiment, with gold leaf, has been tried by Mr. *Franklin*, and many others ; but they seem to be strangers to the cause that produces this effect ; for I do not know any one who has thought of the attracted power going into the leaf gold, &c. without which it would be instantly repelled from the electrified body. For it is not bare matter that is either attracted or repelled in electrical experiments ; but the electric powers which are inherent or adhesive to that matter. But how these different powers do attract and condense each other, or how their atmospheres become repulsive of the same kind, I think, will not be readily discovered ; any more than the cause of gravity and attraction, though the effects are evident in both.

I could go on to explain all the phenomena arising from electrical experiments (which I know of ;) but this would necessarily lead me into a great prolixity, and prevent the pleasure which experimenters have in finding out these discoveries ; in which I think they cannot meet with much difficulty when they fully consider the co-existence of these different powers in all bodies, and their manner

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manner of acting on each other, and that they are equally attracted by all matter.

It may now be expected that I should say something about the medicinal use of these powers; but that cannot be brought into so narrow a compass as many may imagine. For, first, a moderate knowledge of medicine is necessary to know where the effects of these powers may do good or harm, *viz.* in shaking palseys, and inflammatory rheumatisms, &c. they are prejudicial. Next, an accurate knowledge of anatomy, especially in paralytic cases, is very necessary; for, as one sett of muscles revive, an equal attention must be had to their antagonists, or else contractions will be formed, not easily resolvable afterward. And here an accurate knowledge of the action of these powers is absolutely necessary, to know how to conduct them through the parts designed. I was pressed, by a very good physician, who was a patient of mine, and fully convinced of the amazing effects of these powers in his own case, and many other cures which he saw me perform, to write something systematical on the curing of disorders by the electric powers; and carried his compliments so far, as to tell me, that if I let that knowledge die with me, that I should be answerable for it in the other world. But I answered him, if so, so it must be; for that I was then sixty-six years of age, which is a time men naturally grow indolent, except they are warmer in their pursuits than I was.

I may

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I may here observe, that some gentlemen, very well versed in electrical experiments, have published their disappointments in applying electricity to medicinal use. In the 48th vol. of *Philos. Transf.* page 786, Dr. *Cheney Hart* attempted to cure a paralytic arm of a young girl, about sixteen; but, on being the second time electrified, she became universally paralytic; and when the additional palsy was removed by proper medicines; he tried electrifying the second time; on which she became universally paralytic again, and even lost her voice and tongue, and with difficulty could swallow. This made the Doctor cautious in the farther use of electricity. In the 50th. vol. of *Philos. Transac.* page 743, Doctor *William Watson* met with much the same disappointment. In the same vol. Mr. *Benjamin Franklin*, at page 481, says that he never knew any amendment in the paralytic patients, who applied to him, after the fifth day's trial; but that they went home, and in a short time relapsed.

From the great amendment, which Mr. *Franklin* says his patients had in five days time, I think they were all curable, if electricity had been properly understood and judiciously applied. For, tho' I have cured a great number of paralytics, (except in a few recent cases) I seldom knew so great an amendment in five days. But some have continued two, three, four and five months under cure, and have found some amendment

ment every day; and none of them ever relapsed. I have cured some, whom I really thought it was impossible to cure; where the limbs were wasted to skin and bone, and much contracted; yet, from a continued use of electricity, the muscular flesh filled up, and the limbs were restored to their full use. I cured a young woman who was universally paralytic, and had not the least power of motion in any part but her head and neck; yet she was perfectly cured, and has so remained for some years past. Several paralytic patients, whom I cured ten or a dozen years ago, all remain perfectly well; as all that I have since cured also do. I have now under cure an old gentleman, between sixty and seventy, who lost his right side, and his speech entirely, by a hemiplegia; in a few weeks he has recovered the use of his limbs, and in a great measure his speech; and I think that he will be perfectly cured in a little more time. I will mention one rheumatic case, because I think it particular. *Richard Seward* was fifteen years a cripple with the rheumatism, and all parts of his body greatly contracted, that he could scarcely crawl about on crutches; in about five weeks, I restored him the use of his limbs, so that he could walk and run almost as well as ever he could have done, and he has remained well for seven years past.

I have just hinted so much to shew that the good effects of electricity, in paralytic cases, have been permanent; as indeed they have

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have been in almost all the cases, in which I have tried electricity; but if I were to give an historical account of all the cures I have performed, merely by the powers of electricity, it would make a pretty large book; I must therefore here desist.

As I have often said the electric powers condense each other, and have given some experiments which I think sufficiently shew that they do so; yet it may not be amiss to add a few more experiments to make it more evident to others.

Take two panes of clean sash glass, which will lie evenly on each other; place a sheet of paper, folded like a letter, on your table; place the two panes on the paper; and a like paper on the upper pane; then take a large glass tube excited, and roll your tube over the upper paper; repeat the exciting, and rolling the tube five or six times; at the last rolling take up your panes, upper paper and tube together, and immediately take off the tube and upper paper; then bring the two panes, joined together, to a piece of bog-down suspended by silk; you will find the panes, thus joined, have but very little effect on the down; but separate the panes, and each pane will be strongly electrified, the upper pane with the vitreous power on both sides, and the under pane with the resinous power on both sides: touch the down with either pane, and it will be repelled by that and attracted by the other. Place your panes together, as at first, and you will find that the powers condense each other, so
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as to have but little effect on the down : but as you separate the panes you will find the different powers to act as before. The panes, if they lie in contact with each other and are highly electrified, will stick together by their mutual attraction, and separate with a snap. If this experiment is made on a pewter plate placed on a glass stand, you will find that the powers have changed place through the whole; the vitreous power of the tube attracting part of the resinous power from the first glass, and adding part of its own vitreous power to the glass; by which means the glass becomes electrified with the vitreous power; which vitreous power repels the vitreous power of the under glass into the paper and plate, and attracts the resinous power from the paper and plate into the under glass; and thus the under glass is electrified with the resinous power, and the plate with the vitreous power. A due consideration of this experiment may explain all the transactions of the electric powers. For in exciting any electric the rubber always draws off as much of one power as it adds of the other, and is therefore always electrified with a power contrary to the increased power of the excited electric: and when an excited electric electrifies an insulated non-electric, it draws as much of one power from it as it gives of the other; and therefore is itself as much unelectrified, as it electrifies the non electric. And thus this change goes on till the powers become equal to each other; which in non-electrics is done almost instantaneously.

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As I have said that the magnetic powers act by the same laws as the electric powers do, (except that they do not change place from magnet to magnet, as the electric powers do from one body to another;) this may be seen by placing two artificial magnets side by side, with the north and south pole joined together; in which position the powers attract and condense each other, so as to have but little effect on any iron applied to them. Apply two large magnets, so placed to a small key, and they will not take it up; but separate them but half an inch, and they will take it up; but when you close the magnets the key drops from them; which shews that the powers then condense each other.

To shew that while there is an efflux of one power of electricity, there is also an afflux of the other power of electricity, when any non-electric matter is placed so near and in such circumstances as that it can be drawn therefrom. Take a sharp pointed needle four or five inches long, supported on a point like a compass needle, and turn half an inch of each point at right angles with the shank, but contrary to each other, and parallel to the horizon. This needle, when placed upon the conductor while the wheel is turning, will circulate with great velocity, but always from the points, because the electric fire flying off from the points acts forcibly on the air, and is consequently re-acted on, which occasions this motion. But hold this

this needle three or four inches under the conductor, and it will turn in the same manner, by a stream of electricity, of a contrary power to that thrown off from the conductor, which is drawn in from you, and delivered from the points of the needle to the conductor. Let this needle be placed on a glass stand at the same distance under the conductor, and it will not move, because no electricity can be drawn through it; but hold a pin within an inch of the non-electric pedestal, and the needle will immediately begin to turn; a sufficient quantity of electricity being drawn from you through the pin.

This stream of a contrary power from all pointed or small wires held in the hand, and in the atmosphere of the conductor, while the wheel is turning, is evident; but placed on, or in electrics the effect ceases.

In my letters I have said that the sphere of activity of the electric powers is increased by heat; which may require some explanation, and is very necessary to be understood in the rarefaction and condensation of vapour, &c. and even of the air; for it is evident that every particle of the air is attended by the electric powers; because we can electrify the air with which ever power we please, and that either power can be drawn from the air in many experiments. From hence many properties of the air may be explained; viz. its electricity, power of re-

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fracting light, conveying sounds, entering into denser fluids, &c. which I shall not now undertake. When I say that heat increases these powers, I do not mean that heat separates or excites them to attract and repel other bodies by an exchange of powers; as I have explained that all electrifying is performed; but that heat dilates and expands them by rendering them less dense, and consequently more extensive in themselves. This is evident to me in many experiments, but if I were to recite them all I may get beyond the bounds of my readers patience. Take a glass tube and warm it well by the fire, and when excited by friction, bring it under a piece of bog-down suspended by silk, at the distance of two or three inches, and then touch the down at the top with your finger; and withdraw the finger and tube at one time, the down will be strongly electrified with the resinous power; then strike your tube up against the down, you will find the down will stick close into the tube, and the fibres rise one by one slowly till the down becomes electrified with the vitreous power, and is consequently repelled from the tube. But try this experiment with a tube quite cold, and you cannot easily make the down stick to it; which shews that the resinous power, which acts inward to the tube, is much more dilated when the tube is hot, than when the tube is cold. This experiment may be better tried with a pane of glass.

I believe

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I believe that it is unnecessary to say that the rarefaction of vapour, &c. must follow when these powers are dilated by heat, or a condensation when they are contracted by cold; since it is evident that the particles of vapour, &c. are kept at a distance by these powers. But this explains a matter which I think has hitherto lain in the dark, viz. The cause of rarefaction in vapour, &c. by heat, and condensation by cold. And may also explain how vapours may ascend in a hot day, and descend in dew in the cool of the evening and at night, without any exchange of the electric powers in the particles of dew. And may also explain many other things, too prolix to enter upon in this place.

Since electricity does really consist of two different distinct elastic mediums or powers, which co-exist in all bodies, and act in the manner which I have set forth in the following tracts; and that Mr. *Franklin* and all his followers continue to attempt to explain the effects of electricity by one single power; I must beg leave to examine Mr. *Franklin's* pretension to the great honour which he has received for discovering the cause of thunder; which I claim as due to me. For if electricity consists of two different powers, it is impossible to explain the effects by one power; and if electricity is the cause of thunder, it is as impossible to explain that cause without an accurate knowledge of electricity.

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I have already shewn that Mr. *Franklin's* principles are fallible and insufficient, and far from being founded on that evident truth which ought to constitute principles; and that his opinions and assertions, about the operations of electricity in and on glass, are demonstrably false, and that his whole doctrine of electricity depends upon the action of one single medium or power, which never can explain the phænomena which arise from electrical experiments.

In his attempt to explain the cause of thunder, he says that clouds arising from the sea are highly electrified, but that clouds arising from the land are but little electrified; but surely his method of collecting electricity (as he terms it) to electrify vapour arising from the sea, is a very extraordinary one.

He says, the ocean is a compound of water, a non-electric, and salt, an electric perse. When there is a friction among the parts near the surface, the electric fire is collected from the parts below.

I believe that every one versed in electrical experiments must think this notion absurd, and unworthy of so ingenious a man, though it is the basis of his hypothesis. For I am certain that electricity never was excited by whirling a glass globe in a tub of water. As to his opinion about the ascent of vapour; he supposes that the particles of vapour are attached to the particles of air, and

and that they ascend by that means; in which I think that he is entirely mistaken. For, if water, which is a thousand times heavier than air, be joined to air, it will certainly make the mixed heavier than the circumambient air; and therefore the air must rather descend with the water, than the water ascend with the air. Every old woman, who has made pickle, knows that an egg will sink in clean water; and that, when a quantity of salt is dissolved in the water, the egg will swim. And this may serve as an answer to a late treatise, where the author, from a solution of metals in proper menstrua, would prove that a solution of water in air, is the cause of the ascent of vapour, &c.

I now find from Mr. *Priestley's* history of electricity, that an analogy between lightning and electricity, was very early thought of, even before Mr. *Gay*'s time, and very often since his time. If Mr. *Franklin* had any hint of such a conjecture, his proposed method of proving the truth thereof was very rational. For if electricity was the cause of thunder, the only place he could search for it, must be in the clouds; and from his acquaintance with electrical experiments he must have known that metallic rods were the best conductors of the electric power; and therefore, by placing such rods upon electrics, as near the clouds as possible, was a probable method of trying whether such rods would be affected by electricity.

But this experiment, proposed by Mr. *Franklin*, was first tried by Mr. *D'Alibard*, Mr. *De Lor*, Mr. *Le Monnier*, and the Abbe *Nollet*, and others in *France*, and by others in different places; but, to the present time, neither Mr. *Franklin*, nor any of these experimenters have shewn how these metallic rods became electrified, or how electricity could be the cause of thunder. Nor can this matter ever be explained, by those who suppose that electricity consists of but one single elastic medium or power.

That electricity may be found in the clouds, is certain; for it attends all the matter in nature, which we are acquainted with; and, that the clouds may electrify iron rods, &c. in near approach to them, and placed upon electrics, is true; but this is far from shewing, proving, or explaining that electricity is the cause of thunder. For, if only a communication of electricity from one body to another can be said to be the cause of thunder, it may be said that the electricity of the human body is the cause of thunder; as it will electrify other matters: Witness Mr. *Symmer's* black and white stockings. Therefore, a discovery that the clouds are electrified, is not a discovery of the cause of thunder; nor can it be explained how electricity in the clouds can be the cause of thunder, by any thing which Mr. *Franklin*, or others have said about positive and negative electricity, or a plus and minus of the same power, or that glass throws out this power, and that
wax,

wax, resins, and sulphur, &c. drink it in; nor by any thing Mr. *Franklin* has said about charging, or discharging the *Leyden* bottle, where he supposes that as one side of the bottle receives electricity, the other side parts with an equal quantity; and, when a communication is made between the sides, that the emptied side greedily drinks up the abounding electricity from the other side. Supposing this doctrine to be true, I ask any thinking man, how he can from thence prove that electricity in the clouds can be the cause of thunder? For, tho' the *Leyden* bottle gives the strongest indication of an analogy between electricity and lightning, yet there are not any bottles, or glass, in the clouds, to produce this effect, by having one side filled and the other emptied. And Mr. *Franklin* asserts, that the power of the *Leyden* bottle is lodged in the glass, and not in the non-electrics within and without the bottle. How, then, does his increasing the force of this bottle, by adding a number of bottles together, give us any proof that electricity in the clouds is the cause of thunder? Or, if electricity is but one single medium or power, as he asserts, by what known principle of motion is that power put into action? Or, what is the manner of that action, to produce thunder?

I could easily take Mr. *Franklin's* doctrine of electricity to pieces, and shew that it is impossible, from the whole, or any part thereof, to prove that electricity is the cause of thunder; but I believe that it is sufficient to

say that he has never thought of two different distinct powers existing in electricity, or their co-existence in all bodies, or their manner of acting on each other; which all experiments so clearly demonstrate, that I am surprized that such an ingenious man, after all his experiments could have missed this discovery; without which it is impossible to explain the cause of thunder; or any effect arising from electrical experiments.

Mr. *Franklin's* metallic rods, placed from the top of high buildings to the earth, may probably prevent the injuries of thunder, by conducting the electricity of the cloud to the earth; but as he seems ignorant that an equal quantity of a different power of electricity must be conducted from the earth to the cloud to produce thunder, it shews that he is not fully acquainted with the use of these rods, or the cause of thunder; for the electric powers are never rendered visible, except in their passing from one body to another, in opposition to each other; when they condense each other into the form of flame, and perform many of the effects of fire, but more instantaneously than any fire which we are acquainted with. And I am really surprised to find that such a number of ingenious men, after a course of experiments for so many years, have not discovered the co-existence of these different powers of electricity in all bodies, and their manner of acting on each other. Without which knowledge it is impossible fully to explain any effect arising

sing from electrical experiments, particularly the most forcible ones, such as thunder; for these are caused by the great attraction and condensation of the different powers in passing in opposition to each other from one body to another. And if it be duly considered that in all excitation and communication of electricity, there must be a reciprocal exchange of the different powers to each body; it will explain all the phenomena of electricity which I know of. And to this short rule have I reduced it, after all that has been said about it. For the rubber takes as much of one power from the electric, as it gives of the other; by which means the rubber and electric become equally electrified with the different powers. And the same exchange is made in all communication of electricity.

Mr. *Franklin* and all his followers were led into error about the impermeability of glass, and the manner of charging a bottle, by his observing that when a spark was taken into the bottle, an equal spark may be taken from the outside; which made him imagine that there was some stratum in the middle of the glass, which prevented the passage of electricity, and that as one side was filled the other side was emptied; and that when a communication was made between the sides, the emptied side greedily drank up the redundancy of the filled side. But had he known of the two powers, and the rule I have now given for their changing from body

dy to body, he must know that when a spark of the vitreous power is taken into the bottle, an equal quantity of the resinous power goes from the inside of the bottle to the conductor ; by which means the bottle is electrified with the vitreous power ; but when you apply a non-electric to the outside of the bottle, part of the vitreous power goes to the non-electric in exchange for an equal quantity of the resinous power drawn to the outside of the bottle, which resinous power is held there by the attraction of the vitreous power on the inside of the bottle. And thus this exchange of powers goes on as far as the powers can be separated between the globe and cushion.

Place a charged bottle upon an electric stand, and let a piece of bog-down, or cork ball, &c. suspended by silk hang against the outside of the bottle ; when you touch the outside with a finger, the down, &c. remain quiet by the side of the bottle ; but then touch the top of the bottle and the down flies off strongly electrified with the resinous power ; and thus you may go on for a great number of times to alter the balance of the powers within and without the bottle, by alternately touching the top and bottom. Nor can Mr. *Franklin* or any gentleman suppose that it is the return of his plus or positive power to the emptied pores of the bottle, which electrifies the down minus or negatively ? But the truth is, that when you touch

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touch the top you take a spark of the vitreous power from the inside, and in exchange give as much of the resinous power to the inside ; which lessens the attraction of the vitreous power withinside the bottle, which leaves the resinous withoutside in greater quantity than the vitreous withinside, and consequently at liberty to exchange powers with any non-electric in contact with it ; and thus the down, &c. become electrified with the resinous power. Or take two sheets of paper folded like large letters, and place them upon your table, with a pane of clean well dried glass on each paper ; then take a large glass tube, well excited, and roll it over one of the panes, then take up the pane and tube together, and instantly withdraw the tube ; you will find that pane strongly electrified at both sides with the resinous power ; or negatively and minus, according to Mr. *Franklin's* terms ; which he says is a thing that cannot be done. See his letters, printed by *R. Cave*, page 71, 72. second edition. Repeat this operation, and while your pane is electrified with the resinous power, lay it on the other pane for four or five seconds, then take up your panes together, and then separate them ; you will find the first pane still electrified with the resinous power, or minus, and the other pane electrified, at both sides with the vitreous power, or plus. Now if Mr. *Franklin*, or any other man, can frame or strain

strain an hypothesis to explain, by the action of one single power, how a body, divested of its natural share of that power, can give another body a super-natural or additional share of the same power ; I think they will over-set all the rules of reasoning which I know of ; for I think it is a confirmed maxim, that no body can give that, which it has not. But this matter is easily explained by the doctrine I have all along given ; the first pane, having a greater quantity of the resinous power, repels the resinous power from the second pane, to the non-electric under it, and attracts an equal quantity of the vitreous power from the non-electric into the pane ; by which means that pane has an increased share of the vitreous power, which increased power will, by its own elastic force, expand itself into an extensive atmosphere, and therefore the pane will be electrified by that power.

Some gentlemen I think have reasoned themselves out of their senses, and denied the existence of electric atmospheres, and said that Mr. *Franklin's* doctrine would do better without them. But I would ask these gentlemen a civil question, whether it is mere inanity which knocks down steeples and towers, rends trees, tears up the earth, kills men and cattle, sets places on fire, &c. or I might shorten the question by asking how mere inanity or nothing can act ? but this would be a dispute about nothing.

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analogy of explosion
P R E F A C E. *attribution* xlv *form of water*

If gentlemen had been content to confine themselves to facts, I mean such effects as are evident to our senses, I think that they must have necessarily discovered the existence and co-existence of these different powers in all bodies, and their manner of acting on each other, and their being acted on by all matter; which would have prevented their running into such vague conjectures and strange strained hypotheses to explain these effects by a single medium or power; while it is self-evident that two different mediums or powers do really exist. *in the*

If what I have said already is not sufficient to prove the existence and manner of acting of these different powers, I shall always be ready to give any farther explanation, in my power, to any unprejudiced enquirer, who thinks it worth the while to apply to me about what he thinks doubtful.

In one of my letters I have confessed how slender an acquaintance I had with electricity, when I wrote an hypothesis to shew the cause of thunder; for that I had never read a line or conversed with any one on, or about electricity; but that hypothesis, and another on the ascent of vapour, &c. being accepted by the Royal Society; and the only one that stands recorded in the Philos. Trans. and, as far as I can yet judge, as correct as any thing published on that subject; as far as electricity was then understood, or from any thing I have since seen published about

about it; I was encouraged to apply myself to electrical experiments, and to write the following tracts; in which I think that I have demonstrated that electricity is the cause of thunder, as far as the nature of the proofs can possibly admit; and have given a clear and easy doctrine of electricity and magnetism; which will fully explain all the effects, which I know of either. I have often since seen and shewn, to gentlemen in company with me, clouds approaching each other with an increased velocity, by their mutual attraction, and could foretell to them, to a few seconds, when the flash would happen.

It would be an endless task to confute all the false reasonings, and misapplied experiments which I have seen published about electricity; and therefore I must here desist.

But, as I have objected to some of the principal writers on electricity, I suppose that their partisans will object to what I have wrote; but I shall not trouble myself with a reply, 'till I meet with something more explicit and intelligible on electricity, than any thing which I have yet met with.

I readily grant that they may object to my manner of writing; because I never took a note of what I intended to say, nor ever struck out a line which I did write on that subject; except a few in the beginning, where I had said that electricity may be the æther of *Sir Isaac Newton*, before I was sufficiently informed

formed to shew how it may answer his purposes. I know that some gentlemen have taken the characteristics of his æther from a late writing of that great man; where he endeavours to make it the cause of gravity; but I have shewn, in my letters, that such an æther, as he there supposed, could not have answered any of his purposes. I now think that the electric powers, when properly understood, will appear to be that subtle medium which supervades the surfaces of all bodies, and will be found sufficient for the reflecting and refracting of light, and more fitted for that purpose, than any medium Sir *Isaac Newton* could have possibly supposed; for it was not possible for the then known powers of nature, or laws of motion, to suppose the existence of two such subtle elastic mediums or powers, acting in the manner which they do on each other, and their being equally acted on by all matter, till this knowledge was deduced from their effects; which in his time were very little known, and to the present (by all I have seen published) but very little understood.

As to the matter of what I have wrote, it appears true to me, and I believe will do so, to any unprejudiced reader, who will give himself the trouble diligently to enquire into it.

My reasons for publishing these papers are, in the first place, that they will save a great deal of trouble to those who have a mind to enquire

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enquire into the electric powers, and their general laws of action; and then to appeal to the unprejudiced part of mankind, whether those gentlemen of the Royal Society, who call themselves electricians, have done themselves honour, or me justice, in suppressing, and not publishing these tracts, which were addressed to the Royal Society. By which means, they have given Mr. *Priestley*, one of their own fellows, an opportunity of stealing my doctrine, and making it his own, by way of hypothesis, in his history of the present state of electricity; though it is plain, from what he wrote, preceding and consequent to it, that he did not understand it; which I could easily point out through the whole. They have also defrauded me of the credit justly due to me, for the discovery and full explanation of the cause of thunder; and for a clear and easy doctrine of electricity and magnetism, &c. as contained in those tracts. The truth of which doctrine has been verified to me by a very great number of experiments, and a continued application of the electric powers to various purposes, for many years; particularly in medicinal use; by which I have performed many cures in paralytic cases, which our physicians here thought miraculous; and, in truth, they appeared to me in the same light; if a miracle be that, which is above our knowledge of the powers of nature. I know that miracles have been defined to be above or contrary to
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the laws of nature; but I think that nothing can be above or contrary to these laws, which were established invariable, by a Being of infinite Power, Wisdom and Goodness to his creatures.

Lismore, in Ireland, 1769.

* * * These papers are not to be considered as one work, but as the gradual invention of the author, as they are all his own; for everybody was then fond of being first to publish their new discoveries in this science of electricity.

Stockholm printed in 1772



D PHILO-

the laws of nature; but I think that nothing can be above or contrary to these laws, which were established inviolable, by a Being of infinite Power, Wisdom and Goodness to his creatures.

LONDON, in Ireland, 1700.

These papers are not to be considered as one work; but as the gradual invention of the author, as they are all his own; for every body was then fond of being first to publish their new discoveries in this science of electricity.



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PHILOSOPHICAL
ESSAYS, &c.

LETTER I.

Lismore, 20th Sept. 1751.

GENTLEMEN,

THE greatest men of most ages having thought it worth the while to enquire what was the cause of thunder, and the world seeming to acquiesce in an hypothesis subscribed by some great modern names; it must appear presumptuous in me, to offer you some thoughts for a theory intirely new, (at least it is so to me) without I can shew

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that the former hypotheses are ill grounded, and far from being satisfactory. In order to which I shall only object to the latest (to avoid prolixity) which now has the general consent.

I think the basis that this hypothesis stands on, is the authors assuming an analogy between thunder and fired gunpowder; and then proving that there are sulphureous and nitrous particles in the air, they leave them to take fire by fermentation or some other accident, and from thence to form thunder.

First, the analogy is not just; for there is not any thing similar to thunder in fired gunpowder, except the noise. Which may be shewn from the different direction of their fire, and their very different effects.

Fired gunpowder acts, from a center to a circumference, with equal force at equal distance every way, by propelling the circumambient air by the explosion it makes. The fire of thunder acts in rectilinear angles (as I have seen, and as any body may who will observe it) with such subtle and distinct effects, as cannot be explained or imitated by the fire of gunpowder. The history of which effects is too well known to need a repetition here.

I shall go on to shew some insuperable difficulties in the formation and firing this supposed aerial gunpowder. And first I think it inconceivable that the sulphureous and nitrous particles should coalesce with some other unknown third body, in the place of charcoal, in such exact proportion as is necessary to make

make gunpowder of any perfection, and to form a body compact enough to equal the noise of thunder, when fired in the open air. For such a body must necessarily descend by its own gravity long before it arrives to a bulk sufficient for the purpose. And secondly, I think it contradictory to all experience, that such a coalition of nitrous particles should ever happen in the common seat of thunder; which is in the most collected showers which descend; for there the nitrous particles must be absorbed and dissipated in the water; in which state I think it impossible for them to take fire.

These and many other considerations (too prolix for the compass of a letter) induced me to search for some other cause of thunder, which I think I have discovered in that fire which is made apparent in electrical experiments.

This fire pervades and adheres to most bodies, while it flies and cannot be brought to mix with some particular bodies. I shall here only mention two; air which it flies and shuns, and water which it more intimately pervades, than almost any other body. I must also observe that this fire does not only pervade bodies, but that it furrounds and covers them to a certain distance from their superficies in proportion to the state of its activity; which is increased by heat. And that when it is artificially or accidentally protruded upon any body beyond its natural affection, it will fly off to the next approaching body, which is not so much impregnated with this

nitrous fire

fire; and when it departs in any considerable quantity, it makes a considerable noise or crack. All which is demonstrated by electrical experiments.

Now to shew that this fire is the real cause of thunder, we need only consider it attending every vesicle of humid vapour rising into the atmosphere, and covering its superficies to a certain depth, which I think it must certainly do. I shall not here speak my opinion how far this fire is the cause of vapours ascending, because I shall trouble you with that hereafter.

Having got the vapour aloft attended by this fire, without assigning any cause for its ascent, so without assigning any cause for its descent, I shall let it come down as usual, which is in drops much larger than the vesicles in which it ascended. Now in the coalition to form these drops, we must consider what becomes of our fire; for the surface of these larger drops increasing only as the squares, but their solids as the cubes of their diameters, the fire which surrounded the superficies of the vesicles must be protruded to a much greater distance from the superficies of the larger drops, and by that means made more in proportion to the larger drops than its natural affection would have made it join them with, and consequently rendered more apt to fly off to the next approaching or approached body, not so fully impregnated by this fire.

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I have observed before that the constant seat of thunder is in those clouds which are most compact of humid vapour, and which descend in the heaviest showers, and that generally in warm weather, when the adjacent atmosphere is serene; so that the humid vapours are almost all collected into this chain of clouds, where according to the compaction, there will be a body of this fire collected (ready to fly off) sufficient to perform the greatest effects of thunder. Which may be easily computed from the force of electrical experiments; where the smallest portion of this fire, flying off from an electrified body, makes an audible crack, and is able to give a considerable shock. What then must be the force of this fire when it is so collected as to break from a cloud in a body of fire two or three hundred yards in length? which I have often seen.

Now some of these clouds coalescing in their descent, and the drops increasing in their magnitude, there is a vast body of this fire collected more than what would naturally adhere to these drops and their surfaces; which being rendered more active in its vibrations by the heat of the lower part of the atmosphere, the sphere of its affection (pardon the word, for I have no other) is also increased in proportion to the body of fire, which enables it to fly off to clouds (not so much impregnated) at a considerable distance, with that violent crack, so much taken notice of, tho' it is far from being the most wonderful of its effects;

effects; the dire influence of which we often happily escape, by this body's being dissipated by the heat of the lower atmosphere, before it comes within the sphere of its affection for bodies on the surface of the earth.

There is a subsequent rumbling noise heard after the first crack or cracks of thunder, (for this fire does not all break off from one point) which has been taken notice of and oddly accounted for; but I think it neither is or can be more than echoes from adjacent clouds, which at this time are generally dense enough for that purpose; and the noise growing fainter in proportion to the times of its being returned, I think sufficiently proves it.

As to the subtile effects of thunder, I shall leave you to compare them with those of electricity, only allowing for the different force of fire, which is so much greater in thunder than can possibly be procured from artificial experiments; and I believe the analogy will plainly appear. I shall only hint that where one body has been injured by thunder, and another (tho' in contact with it) has remained untouched, that the latter will be found to be of that kind which electrical fire will not join with.

I must beg you will let me know whether this theory is worth your acceptance, for I fear that I am like a fond mother, blind to the imperfections of my own child. I have dandled this opinion these eighteen months past, still fearing to lay it before you. And now instead of seeing its defects, I begin to fancy that

that it has the face of truth and demonstration. If you think this discovery worth the pursuit, I shall venture to trouble you hereafter with some farther attempts, to shew that this fire is a most considerable agent in nature.

First, that the ascent of vapour and exhalation is principally owing to it, and that our atmosphere by that means is kept more homogeneous than is generally supposed, and fitter for respiration, vision, &c. and that clouds of heterogeneous matters are kept suspended at their usual height merely by this fire.

Secondly, I shall prove that this fire is the cause of reflection, refraction, and inflection of light.

Thirdly, I shall endeavour to shew that this fire is the cause of that secondary attraction and repulsion, that Mr. Newton has taken notice of.

Lastly, I shall give some hints of the great uses of this fire in animal life and vegetation. What further I have thought of this fire I shall not now trouble you with. I am,

GENTLEMEN,

Your most humble and

most obedient servant,

HEN. EYLES.

* * Sent this letter dated 18th June, 1752, to the President and Council, of the Royal Society in London.

The

The ANSWER to the First Letter.

S I R, *London, Nov. 25th, 1752.*

YOUR letter, of the 18th of *June* last, to the Royal Society, concerning the cause of thunder, came to my hands after their summer recess, so that it could not be presented to them 'till after the resuming of their weekly meetings this month; on the 16th of which I read it to them, and received their orders to return you their thanks for it, and their desire of your thoughts upon the other subjects which you mention.

Your hypothesis, with relation to thunder, is justified by the experiments made in *May* last, and several times since, upon thunder-clouds in *France*, and verified by others in *England*; of which the world will have full satisfaction in the volume of the *Philos. Transactions* for the present year, now in the press, and to be published about *February* next.

Any of the future communications, with which you shall favour the Society, will find me at my house in *Norfolk-street*, in the *Strand*, or at that of the Society, in *Crane-Court, Fleet-street*. I am,

S I R,

Your most obedient humble servant,

To Henry Eccles, esq; THOMAS BIRCH,
at *Lisamore*. Secret. RS.

LETTER

LETTER II.

SIR, *Lismore*, 10th Nov. 1754.

I RECEIVED your letter of the 25th Nov. 1752, in answer to my letter to the Royal Society, concerning the cause of thunder. As you tell me that they have honoured me with their desire of my thoughts on some subjects mentioned therein, I have ventured to send you a letter for them, on one of these subjects, inclosed in two packets, by this post.

I fear that the Society will think my letter too hypothetical; our great modern philosopher having in some measure condemned the use of hypotheses. But I must boldly observe that, that objection is made in a part of his works which is entirely hypothetical; I mean his *Queries* at the end of his third book of *Optics*. And I shall venture to shew (if the Society will have patience to hear me) that there is a great deal hypothetical in his second book of *Optics*; where he thinks himself more ascertained. For he there speaks pretty positively of four causes for the refraction, &c. of light; three of which must be wrong, and the fourth, (his æther) in the manner he has proposed it, can never be sufficient for the purpose. Not that I intend impudently to blame that truly great man, but to endeavour to investigate the cause of reflection and refraction of light, by means not made sensible to mankind in his time.

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I may also observe, that, if his hypothesis had not preceded his calculation, the world had never been obliged to him for his great doctrine of attraction. For at first he only supposed the power of gravity may extend to the moon; and that she may be retained in her orbit thereby; but when he made his calculation, (having mistaken sixty *English* miles for the measure of a degree on the surface of our earth) he thought that some other power must co-operate with that of gravity, to keep the moon in her orbit; and, for that time, laid his attempt aside; 'till *Picart*, in *France*, measured a degree, and found that it contained about sixty-nine *English* miles and an half; and then, or some years after, our great author, having resumed his computation, found that the power of gravity was alone sufficient to keep the moon and planets in their orbits; and thereby proved his hypothesis.

Indeed, in works of invention, I can't see how it is possible to proceed without something hypothetical; for, the supposition must ever precede the proof. I am not for establishing hypotheses as principles; but if an hypothesis is rationally founded, I think it is worth the while to enquire into the truth of it, and thus invention may proceed. I shall not say any thing in justification of my own, because I have not any vanity to gratify; for, if my letter has nothing worth their, or your acceptance, I can most readily and willingly ask pardon.

don for the trouble I have given, and desist from giving any for the future. I am,

S I R,

To the Rev. Mr. Thos.

Birch, Secretary to

the Royal Society, in

Crane-Court, Fleet-

Street, London.

Your most obedient,

humble servant,

HEN. EYLES.

L E T T E R III.

GENTLEMEN, *Lisimore, 16th Nov. 1753.*

THE great honour you have done me, in accepting my letter concerning the cause of thunder, has emboldened me to proceed, in part of what I therein proposed, *viz.* to endeavour to shew, that the fire, which is made apparent by electrical experiments, is the principal cause of the ascent of vapour and exhalation; and that the lower part of our atmosphere is, by that means, kept more homogeneous, than is generally supposed, and fitter for respiration, vision, &c. and that clouds of heterogeneous matters are kept suspended at their usual height, merely by this fire.

To which I have added something, concerning the cause of the winds; and to explain the general phenomena of the weather and barometer.

But, as I must now contradict some generally received opinions, it may be thought decent to make some apology for offering my own; but I shall not take up your time in so doing;

doing; for, if they carry an appearance of truth, I need not make an apology for them; if they appear false, they cannot injure those opinions which they contradict; and then I have but one apology to make, which is, to acquiesce, and sincerely ask your pardon for having troubled you with them.

It is agreed that the ascent of vapour and exhalation through the air may be effected two ways; by impulse, and an alteration of their specific gravity.

That vapour does not generally ascend by impulse, may be proved by many familiar experiments, *viz.* Put boiling water into a vessel, and then empty it, and hold the vessel with the aperture downwards; the vapour which is afterwards expelled from the vessel must be in a direction downwards; but we find that, as soon as it has got but a very little below the rim of the vessel, it has its direction altered, and ascends by the laws of specific gravity. The same thing may be observed in all boiling vessels where the vapour is emitted in a direction downwards; or in cold weather, when the vapour of a man's breath may be seen, let him breathe downwards, and the direction of his breath will be presently altered, as in the former case.

Since vapour does then ascend without any other impulse, than that which is incident on all bodies, ascending by the laws of specific gravity, it is necessary to enquire how the specific gravity of vapour is altered to cause its ascent.

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This is generally supposed to be done, by filling vesicles of water with rarified air, until the diameter of the vesicle be ten, or more times, the diameter of a drop composed of the same constituent particles of water; and that the vesicle by this means becomes specifically lighter than air.

But I think that this cannot be done so easily as it has been generally imagined; and, when done, it will not be sufficient for the purpose: which will appear from the following considerations. First, the great difficulty in forming those vesicles, especially of the particles of dry bodies, carried off by exhalation, and filling them with rarified air, while the exterior air remains condensed. Secondly, that there is not any allowance made for the weight of the included air. Thirdly, the constituent particles of water are but very little (if at all) altered in their specific gravity. Fourthly, that this thin vesicle can never be a sufficient boundary between the exterior condensed air and the interior air so exceedingly rarified. And therefore as the density of air is proportional to the force compressing it, it follows that the interior air must be suddenly compressed, and the vesicle with it, until it be of an equal density with the exterior air, in which state the vesicle is not more fitted for ascending, than if the same constituent watry particles were formed into a spherical drop.

For here I must beg leave to set aside a false reasoning which I have found in many approved

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ed writers on this subject, who assert that the specific gravity of bodies is altered by a mere encrease of surface upon their being divided into minute parts. Which is wrong. For the specific gravity of any body is only to be altered by making that body occupy a greater or less portion of space. Now it is evident that upon dividing a body into parts, the surface encreases according to the number of parts, but the space occupied, and consequently the specific gravity, remains the same. Take a cube whose side is ten inches, the surface is six hundred square inches, the space occupied one thousand cubic inches; divide that cube into a thousand cubes; the surface will be encreased to six thousand square inches, but the space occupied will remain a thousand cubic inches, and therefore the specific gravity will be the same; and so on divide it into as many parts as you please. Indeed bodies, by an encrease of surface, meet a greater resistance in passing through any medium; but I think resistance and specific gravity are here carefully to be distinguished. For that resistance which prevents the sinking of the minute parts of a body, in a fluid specifically lighter, must equally retard their ascent in the same fluid; and therefore can never be the cause of their ascending.

But let us suppose that this vesicle is formed and filled with rarified air, and ascending through the atmosphere; it is plain, from another cause, that it cannot remain so during a few seconds of time: for no solar or culinary heat can expand air, so as to make it permanent

manent in that state, in a vesicle of vapour, ascending through the open atmosphere, for the space of one minute; for it is evident, that metallic bodies, heated many degrees hotter than boiling water, and exceedingly larger than the ascending vesicles, cool in a few seconds, when exposed to the open atmosphere. For instance, mechanic operators, in tempering small drills, only heat them red hot in the flame of a lamp, and then waving them three or four times through the air, find them not only cold, but hardened by their sudden cooling, so as to be fit for cutting other parts of the same steel, which has been cooled more gradually. Now, the vesicles, being exceedingly smaller than the points of such drills, must cool much quicker, and the included air be condensed equal to the circumambient air almost instantaneously; in which state the vesicle is not fitter for ascent, than that portion of water was before the vesicle was formed; for though the surface of the water is vastly encreased by the form of the vesicle, which may retard the descent of it through the air; yet that form must equally obstruct its ascent; and the specific gravity of the constituent particles remaining the same, I can't see how the encrease of surface can aid its ascent. And here I must observe, that it has not yet been explained how water can be dilated, so as to occupy eight hundred or a thousand times the space which it naturally does. For I think the greatest expansion it suffers by boiling (the greatest heat

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we can give it) is not more than a nineteenth or twentieth part of what it was before.

But, supposing these vesicles formed and ascending; there is still a very necessary power wanting, I mean repulsion, to prevent their running into contact (by their natural attraction) and forming larger masses; in which state they would immediately descend.

Now let us suppose that they ascend by impulse; it will be very difficult to explain their motions which are evident to sense. For if they ascend by impulse, their motion must be continually retarded by their gravity and the obstruction of the air, until they arrive at their greatest height, which must always be in proportion to their first velocity; and then they must descend by the laws of specific gravity, as all other bodies do. The velocity with which we generally see vapour ascend, is not sufficient to carry it a few yards in height; and therefore, if there was no other cause of ascent but impulse, the vapours and exhalations must be pretty equally diffused over the surface of the earth, ascending to a small height, and immediately descending again: Or, if it were possible for them to float at that height, the consequence must be a perpetual fog. How far the great uses of vision would be obstructed or lost in this case, I need not mention; nor need I say how detrimental it would prove to respiration, since mankind have agreed to call fogs unwholesome. However, when I come to speak of the uses of electrical fire in animal life, I shall say something on this subject.

However

However necessary an impulsive power may be in the first emission of vapour and exhalation from their masses, I think it evident, from the slowness of their ascent, that it cannot carry them to that height where clouds are usually formed; much less is it possible that it should be the cause of their floating there for many days, as we see they do. This can only be done by an alteration of their specific gravity, till they are brought into equilibrio with that part of the atmosphere where they float.

It now remains to enquire by what means this may be done; since neither impulse, rarefaction of the air, or any formation of their parts by expansion (which I know of) seem sufficient for the purpose.

There appears to me but one way of altering the specific gravity of the particles of vapour and exhalation to render them lighter than air; which is by adding to each particle a sufficient quantity of some fluid, whose elasticity and rarity are exceedingly greater than that of the air. That the fluid or fire of electricity is such, I believe will be easily granted me; but how far it is adapted to this purpose, we must enquire from experiments.

But, first, let me not be thought absurd in saying that the Creator might have thought proper to adapt this fluid to this particular purpose; for the purpose is great, no less than all vegetation and animal life depending on the ascent and descent of vapour and exhalation.

I need not run into a prolix detail of electrical experiments to prove the properties of this fire, which are much better known to you. Altho' I have made some experiments, which, perhaps, have not yet come before you ; by which it appears that all fumes arising from fire, whether blazing or otherwise, and all steams rising from boiling or warm waters, and from all other fluids, and the breath of man and of all other animals, and all the effluvia thrown off by perspiration, are all strongly electrified ; but I shall not trouble you with these until I come to speak of the uses of this fire in animal life. I shall now only mention a few which are well known, and which are to my purpose. First, that desultory motion by which it flies off from an electrified body to any number of non-electrics which are brought within the sphere of its activity and affection, until it be equally diffused through all. And, Secondly, that the sphere of its activity is encreased by heat. Thirdly, that this fire does not mix with air. Fourthly, that it intimately pervades water, and many other bodies, covering their superficies to a certain distance ; which distance is not in proportion to the bulk of the body electrified, but in proportion to the state of activity of the electrical fluid. Fifthly, this electrical fluid readily joins with any fire which fumes, or rather with the blaze, or fumes of any fire ; but will not mix or fly off with the fire of red-hot iron, or any other metal which does not fume. I have not met with this ob-
servation

servation in any writer on electricity, but I have proved it by experiment.

Now to shew that this electrical fire or fluid is the principal cause of the ascent of vapour and exhalation, we need only prove that it attends all vapour and exhalation, and that in such quantity as is necessary to render them specifically lighter than the lower part of the atmosphere.

I shall not undertake to determine by what cause vapour and exhalation are detached from their masses; whether by the solar or culinary fire, or by the vibrations of the electrical fluid rendered more active by those fires; tho' I am led to think the latter. But it is evident that they are ^{em}itted in exceeding minute distinct particles, and that these particles must pass through that electrical fluid which surrounds the surface of the mass, and that, by that means, they must be equally electrified with the mass; that is, they must be covered with the electrical fluid to as great a distance from their superficies as the mass is covered; which must always be in proportion to the state of activity of the electrical fluid. In which state, when they have passed the surrounding fluid, they must be repelled by it, and also repel each other; and if each particle of vapour and its surrounding fluid occupy a greater space than the same weight of air, they must be fitted to ascend 'till they come in equilibrio with the upper and rarer part of the atmosphere, where they must float until their specific gravity is altered. As it is very difficult to as-

sign the magnitude of each particle of vapour and exhalation, and that of the surrounding fluid, and to shew that both taken together occupy a greater space, than the same weight of air; we can only apply to experiment to shew that it is possible that it may be so; and that will shew that in all probability it is so; since it is evident that every particle must be endued with a portion of this electrical fire or fluid; and that there is not any other sufficient cause assigned for their ascending. It is evident that upon electrifying any light matter, such as down or the downy parts of feathers, that their specific gravity is much lessened, and that by holding another electrified body under them, they may be driven upwards at pleasure. It is also evident from experiment, that the more you divide the parts of such bodies, the more of their specific gravity will they lose by being electrified; and by dividing them into minute parts, I have found, that they ascended to a considerable height after they were electrified. From whence I think it highly probable that the exceeding small particles of vapour and exhalation may be, and are sufficiently electrified to render them specifically lighter than the lower air, and that they do ascend by that means. And that they will ascend proportionally higher as the surrounding fluid is proportionally greater than the particle which is carried up.

It may be now thought necessary to shew what becomes of our vapour and exhalation above, or how they are to come down again.

But

But I must here beg your patience while I endeavour to shew that the ascent and descent of vapour and exhalation attended by this fire is the principal cause of all our winds. In doing of which I hope to bring down the vapours and exhalations, and explain the general phenomena of the weather and barometer.

It being admitted that wind is only air put into motion, many have been the conjectures how that motion is caused. Among which the motion of the earth, and the air's being rarefied by the sun seem to stand first. The trade winds being most regular, and occupying a considerable part of the globe, it has been thought proper first to account for them from the afore-mentioned causes. But I think that these causes, by themselves, are not sufficient for the motion of those winds, and much less so for the irregular motion of all the other winds. If the apparent motion of the air was occasioned by the diurnal revolution of the earth from west to east, by the air's being left behind, the motion must be found more regular, and very different from what it is; for in that case the greatest motion must be at the equator, and from thence lessen by regular degrees to the poles, and must be continued always equally one way, both day and night, and at all seasons. But we find quite the contrary, the most gentle gales blowing at the equator, and between the tropics, pretty steadily one way all day long, and dying away at night; while high winds and storms,

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blowing all manner of ways, are found in the higher latitudes.

I shall not detain you to shew that the sun's rarefying the air cannot simply be the cause of all the regular and irregular motions which we find in the atmosphere ; but I shall proceed to what I think is the cause, *viz.* The ascent and descent of vapour and exhalation attended by the electrical fire or fluid.

You are well acquainted with the calculations (by sundry authors) of the quantity of vapour raised in a day ; but I will venture to say, that all those I have met with fall exceedingly short of the quantity raised in the torrid zone. Now all this vapour and exhalation being buoyed up by the electrical fire, must add a column to the air, tho' of a different matter, at least a thousand times greater than the vapour and exhalation taken up ; which column must necessarily force the adjacent part of the incumbent air upwards ; and must as necessarily be re-acted upon by the incumbent air, to restore the equilibrium of the whole air. And as it cannot be readily forced down again, it must float off, at that altitude, toward those parts where little or no addition has been made to the atmosphere, and by that means must propel the air on the horizontal level with it, and that below it as it is itself propelled by the weight of the incumbent air ; and that motion must be from the equator, where the greatest quantity of vapour, &c. is raised, toward the poles, and partly to the west, as the earth turns toward the sun. For here we must
confess

confess that the sun is the great agent in detaching vapour and exhalation from their masses; whether he acts immediately by himself or by his rendering the electric fire more active in its vibrations. But their subsequent ascent I attribute entirely to their being rendered specifically lighter than the lower air by their conjunction with this electrical fire.

The fire which surrounds the vapour beginning to condense, and the vapours to subside in passing the tropics becomes a greater pressure on the air beneath, and by that means forces some part back into the tropics in the place of that air protruded by the ascent of the vapour, &c. and the remainder in a direction toward the poles. The common rotation of the air in coming in below to supply the place of that part carried up by any fire may explain this motion.

To shew how this motion must tend to the west, we must consider that the column of air, raised by the ascending vapour, &c. is at its greatest altitude to the east; and therefore must press that air to the westward which is continually protruded by the vapours, &c. beginning to ascend from east to west; and the compressed air at the tropics must tend to the westward, 'till their forces ^{meeting} make the motion intirely to the west. The air itself being rarefied and carried up by the reflection of the intense heat of the sun (for heat in flying off from all bodies does ascend through the air with great velocity) may be a considerable

able additional cause of these trade winds ; but never can be the sole cause of all the erratic winds.

As I am not sufficiently informed of the particular formation of the globe, to account for all the irregular winds within the tropics, I shall only venture to say that where such happen, it must be by means of some tracts of lands which rise to a greater height above the horizontal level, than vapours generally do ; by which the motion of the vapour is stopped, and the vapour accumulated by succeeding vapour, and the air upon which they float is of consequence pressed into a new direction.

And from hence may also be explained the cause of the rains, particularly so called in the sea language.

I must now consider what becomes of the vapour, &c. floating from over the tropics toward the poles, which being less affected by the heat of the sun, reflected from the surface of the globe, the surrounding electrical fire begins to condense more and more as it moves toward the poles, and the vapour of course to descend, and that part most, which is most remote from, or is farthest left behind by the sun ; and of consequence the higher column of air must tend that way to restore the equilibrium, which motion, at this side the equator, must be to the north-east ; and as the vapour, &c. fall again to the earth the motion must be more to the east. From whence
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our south-west and westerly winds, which blow a considerable part of the year. But as this system is too regular to account for the phænomena of the erratic winds, we must consider whence they arise. I have before observed that tracts of land rising into the atmosphere will stop the regular motion of the vapour, &c. and that the vapour being accumulated by succeeding vapour, the subjacent air must be pressed into new directions. Now this cause added to the daily dilatation of the electrical fire, and the contraction at night, and the coalition of vapours to occasion their total descent, will be sufficient to produce a very great variety of winds on this side the tropic. But if I were to set down all that has occurred upon this subject about upper currents, under currents, and intermediate transverse currents, &c. occasioned by the various motions of the atmosphere to restore an equilibrium; and, to produce my proofs, I must send you a book instead of a letter; but I write not to common minds.

It now remains to shew how the general phænomena of the weather and barometer arise from this system. First, why it generally rains in the winter, while the wind is south, south-west, and westerly. Secondly, why north-west winds are generally attended by showers in the beginning, and become more dry, as they are of longer continuance. Thirdly, why north and north-east winds are generally dry. Fourthly, why the east wind continues

tinues dry and dark for a considerable time together. Fifthly, why squalls precede heavy and distinct showers, and a calm ensues for some little time after they are past. Sixthly, why storms and high winds seldom happen in a serene sky without clouds. Seventhly, why the vapours in warm seasons coalesce to form those distinct, dense clouds, which produce thunder and heavy showers. Eighthly, why the barometer falls lowest in long continued rains attended by winds, and why it rises highest in long continued fair weather; and why the intermediate changes happen. Ninthly, of land and sea breezes, and water-spouts.

First, the vapours, passing the tropics into the colder regions, have their surrounding fire condensed by degrees, which must encrease their specific gravity, and lessen their repulsive power; by which means they must both descend and approach each other, 'till at last they form dense, visible clouds; and these clouds are also accumulated by other succeeding vapours, of like specific gravity, 'till they form clouds, which are often several hundred yards in depth; which I have often seen, in passing through them up the sides of very high mountains. In clouds of such depth, I think the coalition of their particles, to form drops, may arise from their motion, and the order of specific gravity. For, the air being a great deal denser, and of greater specific gravity, at the bottom of such a cloud, than it is at the top; and the particles of vapour, with their fire, being pretty near of an equal specific gravity; those

those particles which are below must necessarily be forced upwards, and those above must as necessarily descend, 'till they all meet at that altitude, whereat their specific gravity would make them float; if they were not prevented merely by their repulsive power; but this repulsive power must be greatly acted upon, if not quite overcome, in some places by this pressure of the air and of the vapours. But supposing the particles only brought into a very near approach by this pressure, it will be scarcely possible for them, upon any motion of the clouds, to keep their stated distances, without impinging upon one another: And whenever this happens, it is easy to shew how rain is formed. For, wherever two or more particles are forced to approach within their repulsive powers, they will run into contact and sphericity, (if fluid) by their attractive power, and by that means become covered with the electrical fluid to a greater distance from their superficies, than the other particles are, or than they were before their conjunction; (as I have shewn in my letter concerning the cause of thunder) that is, they will be more electrified; and consequently, as appear by electrical experiments, the excess of electrical fluid will run off among the other particles; by which means the enlarged particles have their specific gravity encreased, and are enabled to descend to a lower region of the air; and the more particles they impinge upon in their descent, the more will their specific gravity and velocity

velocity be encreased ; and the more their velocity is encreased, the more particles will they imping upon, till they fall from the clouds in drops, whose bigness will be according to the depth and density of the cloud they have passed through.

It may be necessary to shew, why I assert that the encrease of velocity in the enlarged particle will make it imping upon a greater number of the smaller particles, in its passage through them. And this will appear by electrifying a feather, which may be driven upwards, by the electric body, with a certain velocity ; but if you add a much greater velocity to the electric body, it will overtake the feather, and get within their repulsive power, and the feather will stick to it by their attractive powers. And the less distance the feather keeps above the electric body, the less encrease of velocity is necessary to make them join. You'll pardon my prolixity in this particular ; because I think much depends upon it, in the formation of rain.

Having shewn how I think vapours may coalesce into rain, I shall now endeavour to explain the phænomena of the weather, &c.

And first, why our south, south-west, and westerly winds are wet in winter, I have before shewn that the wind, and vapour with it, must tend from the tropic toward the pole, and how it may be varied to the north-east, and to the east. In the cold winter season, the vapour, in this course, must coalesce more and more as it comes into the colder regions, and
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at last come down in mists and rain; and the more the column of air is decreased by the fall of such mists and rain, the more room is there for succeeding vapour to be pressed into the same course, and to fall in like manner, and so on, which may occasion the continued course of these winds and wet, which generally attends our winters. But sometimes at the fall of these rains, the wind comes in from the north-west, to restore the balance of the atmosphere, and rolls the vapour, still floating in the air, (by altering its motion) into heavy dense clouds, which, upon their sudden coalescence, let fall heavy showers; and by that means the remaining particles of vapour in the cloud have a greater quantity of electric fluid distributed among them, which enables them to ascend, and form what the sailors call an hard dry sky; and as they approach farther into a warmer climate, which encreases the power of the electrical fluid, they rise, dissipate and vanish out of sight; and this is the general consequence of north-west winds. As to the north and north-east winds, whatever vapour they bring with them, has its repulsive and ascending power (that is the electrical fluid) so encreased by approaching into warmer and warmer climates, that it scarce ever comes down in rain, except from the effects of some upper current of the air.

It sometimes happens, while the wind blows very gently at east, that vapours coalesce at a considerable height in the atmosphere, so as to form one even, uniform cloud, sufficient to
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shut out the bright shining light of the sun, which is therefore called a dark sky; which cloud being uniform, and of no great thickness, and carried on by a very gentle motion, and that neither approaching a warmer or a colder climate, and seldom descending so low as to be disturbed by the tops of mountains, the vapours may keep the order of their specific gravity for a considerable time, as they generally do; till some cross or opposite wind forces them to separate into denser clouds, and leave apertures for the sun to shine through; and this is generally the case for a day or two before any wet descends.

Why squalls precede heavy distinct showers, and a calm ensues for some little time after they are past, is pretty evident; for the descending rain, attended by the electrical fluid, forces the subjacent air out of its place, and that which is driven forward must add a proportional velocity to the motion of the wind that way, and that which is driven backward must also obstruct the motion of the wind advancing after the cloud. The wind preceding the cloud is also pressed off obliquely toward either extreme of the shower. Which may be of good use for sailors to know and observe; for if they sail upon a wind from the center of the shower toward the extremity, they may safely venture to keep their luff; but if they sail from the extremity toward the center, if they luff up, they will be taken a-back, and run the hazard of being dismasted; and this I always found true in practice.

Why

Why high winds are seldom found in a serene sky without clouds is also pretty evident; for clouds occasion these high winds in a double manner; first, when they are large and united and upon the descent, as they occupy a great space in the atmosphere, they must press the subjacent air into a great velocity; and secondly, when they coalesce and come down in heavy rains, they make room for the air to flow in with violence to restore the equilibrium of the whole air.

To know how the vapours coalesce in warm seasons to form those distinct dense clouds, which produce thunder and heavy showers, we must first consider the state of the vapour in its ascent, which must be strongly endued with the electrical fire to enable it to ascend to the great height, it then does, in the atmosphere; secondly, the great quantity carried up and kept afloat at that great height during a series of hot dry weather. The quantity may be proved from the height of the mercury in the barometer (as I shall shew presently) and the great height and dissipation of the vapour from the serene appearance of the sky; and also by the great height where even thunder clouds are formed in its descent; which may easily be measured by the length of time between the light and noise of thunder. In this state the upper air is in a quietude, and whatever part of this vapour begins to coalesce and subside first, will carry down with it part of the subjacent vapour, and make room at the top for other vapours to

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flow

flow in from all sides, which, by their meeting, may also coalesce and subside to form this central descending cloud ; and so also of like succeeding vapours. And thus I think clouds of any density may be formed, in a quiet air (and thunder generally happens in calm weather) 'till the density is too great for the buoyant electrical fire to sustain the weight ; which flying off in part from the largest drops, lets them fall almost in spouts.

Why the mercury falls and rises in the barometer will appear from what I have been saying ; for in long continued fair dry weather there is a great quantity of vapour raised and kept afloat in the air ; which must necessarily add a column to the air at least a thousand times greater than the vapour taken up, and oftentimes much greater than that, which must as necessarily encrease the weight of the air, and make the mercury rise proportional to the additional column ; and when rains descend, they subtract a column from the air near a thousand times as great as the water fallen, which must necessarily lessen the weight of the air, and make the mercury fall proportional to such subtraction.

As to winds, they affect not the mercury, but as they are generally, in these climates, a consequence of descending clouds and rains, blowing into those parts where the column of air has been lessened, and while they blow, the mercury will continue low in the barometer, 'till the equilibrium of the atmosphere be restored. But I believe if it were
enquired

enquired into, whether the mercury descends when the wind begins to blow every morning between the tropics and the vapour to ascend, that it will be found that the mercury rather ascends.

There is a phænomenon which has puzzled many, that is why the mercury falls before the rain falls. This may be explained by the column of air's being lessened gradually, by the condensation of the electrical fluid from the first descent of the vapours till they fall in rain.

I shall now say something of land breezes and sea breezes, a phænomenon which sometimes happens in fair settled weather; that the wind blows out from the land at night, and in from the sea at day-time. The land breeze is occasioned by the descent of clouds and the particular formation of the land; for if the land rise into an hilly country from the sea, when the clouds and vapours descend at night, which they often do by the electrical fluids being condensed, they must press the air down the land toward the sea in their fall; as may appear from the smoke of any fire running down the side of an hill, in the evening of a damp day when the clouds are upon the descent. And the sea breeze is occasioned by the clouds ascending in the day-time, which must impel the incumbent air upwards, and make room for the sea breeze to flow in; but beside the mere ascent of clouds there is an exceeding greater quantity of vapours raised from the land than from the sea. For the same extent

of land has an exceeding greater surface than the same extent of sea ; which may appear from the various forms of vegetables and animals, &c. and the greater the surface the greater will the evaporation be ; beside the more irregular these surfaces are, the greater will be their reflection and refraction of the sun's beams, which will encrease their power. And it is also necessary that the evaporation should be much greater from vegetable and animal fluids, than from fluids in a quiescent state, to carry on a circulation for the great work of nutrition. But of this when I come to speak of vegetation and animal life. Now the ascent of these vapours must beget a circulation of the air inward from the sea ; in the same manner as the ascent of vapours from any fire brings in the air below to that fire.

- As to water spouts I have found them oddly described by the learned ; as being great columns of water sucked up from the sea by the clouds. But I never saw any such ; nor could I find upon enquiry from many honest men, who have sailed almost all our known seas, that they ever met any such ; and therefore I do not believe that there are any such. There is indeed an appearance something like their description, which may have given rise to their conjectures ; but this is no more than a very heavy shower from a very dense cloud, which is drawn into a conical form, and a very narrow compass at bottom, before it arrives at the sea ; which it dashes with great violence in its fall. It may be worth the while to

to enquire how the shower comes into this conic form ; which I think is caused by the general attraction. For while the vapours floated in the air, being of like specific gravity, they were equally attracted by the air as by one another ; but when their specific gravity was greatly encreased by their being formed into larger drops, and letting go great part of that elastic fluid which buoyed them up, and the greatest quantity of water falling in the center of the shower, the extremes may be greatly attracted to the center, in their passage downwards.

There is one objection may be made to all I have been saying of the winds and weather ; that is the great distance of the tropic, and that the wind must be a great time in coming to us from thence. But this objection will lessen, when the velocity of the wind is considered, which in a fresh gale will move above a degree in two hours ; which will soon bring it from the tropic to us. And this velocity, and a much greater, I proved by a machine, I invented and made in the year 1733, to go upon wheels by the force of the wind. For I have been carried in it, at the rate of more than a degree in two hours, when the wind was upon the beam, that is when I could sail forwards or backwards upon the same tract with equal facility ; so that my motion, in going before that wind, must have been much greater ; which I then neglected to estimate.

And now, gentlemen, I fear two things, that I have said too much, and that I have said too

little; for if this letter appears with the same face of truth to you as it does to me, I have omitted many things, for fear of being too prolix, which I ought to have added; but if I have written without a foundation in truth; I must, long before you come to this part of my letter, appear prolix and impertinent; and therefore it may be time to conclude; before I do which, I will assert one thing, that is, if any thing in this letter is found worth your acceptance, that it is intirely my own; for I have not borrowed a single hint; no more I did in my letter concerning the cause of thunder; for when I wrote that letter, which was on the 20th *Sep.* 1751, I did not know that any one had made the least conjecture that the electrical fire was the cause of thunder. And I had told many gentlemen in this kingdom, in the beginning of the year 1751, that I thought I had discovered the real cause of thunder. Indeed immediately after my sending that letter to you on the 18th of *June*, 1752, I met with some accounts in our news-papers that iron bars, some how or other, set up during the time of thunder, were found to be electrified; but from that time to this I know nothing particular of those or any other experiments on thunder clouds. I waited the publishing of the volume of *Philos. Transactions* for the year 1752, to inform myself of these matters; but I don't find that it is yet come to our kingdom. As I am not a plagiarist, so I would not willingly be thought one; nor should I venture to trouble
you

you with what I thought you may have better from other hands ; but as the matter of this letter appears new to me, so I venture to lay it before you, without any other motive than to add my mite in the investigating truth.

I am,

GENTLEMEN,

Your most humble and
most obedient servant,

HEN. EELES.

My fears of trespassing upon your patience had almost made me forget to explain how far the wind assists vapours to rise in greater quantities and in less time than they would in a quiet air. And this omission would not be excusable, because both the learned and unlearned have observed that a greater quantity of vapour is carried off in windy weather, than when the weather is calm. When vapours are ^{em}itted in great plenty from the surfaces of fluids, such as from a pot before it begins to boil, or from the surface of a bowl of hot punch, &c. it often happens that they crowd each other in such manner that the surrounding electrical fluid of each particle is compressed, and cannot occupy that portion of space which is necessary to make it buoy up the particle ; and thus the particles float in a steam just above the surface of the liquor, and they are prevented from falling back into the

liquor by the electrical fluid on the surface of the liquor. In which state, none but the uppermost particles can ascend; which they do by the elasticity of the electrical fluid forcing them upwards, till they have room for the electrical fluid thoroughly to expand itself, so as to be able to buoy up the enclosed particle of vapour. In this state the assistance of the wind will be easily understood; for the wind blowing them from the surface, dissipates them and gives them room to ascend, and clears the surface to make room for the succeeding vapours to ascend, and so on; which in this case will greatly aid the ascent of vapour. And thus far I think the wind assists in the ascent of vapour, but no farther. I have mentioned hot liquors only, because the steam is more visible in them, not but the same case often happens in evaporation, where we cannot so easily perceive it.

To the President, Council,
and Fellows of the Royal
Society.

* * Sent this letter, dated the 10th of November, 1754.

The

The ANSWER to the Second Letter.

S I R, *London, March 6th, 1755.*
I HAVE deferred my acknowledgments
 for your letter, and two packets directed
 to me, 'till I had read your papers addressed
 to the Royal Society, who return you their
 thanks for them. It would be a further ob-
 ligation on them, if you would favour them
 with the experiments made by yourself, to
 which you refer for the support of your
 reasonings.

Your communications will always be
 highly acceptable to them; as any of your
 commands will be to,

S I R,

Your most obedient, and
 most humble servant,

To Henry Eeles, esq;
 at *Lismore*.

THOMAS BIRCH,
 Secret. R. S.

N. B. No answer to, or any notice taken
 of, any of my other letters; tho' repeatedly
 required by me.

HENRY EELES.

L E T.

L E T T E R IV.

SIR, *Lismore, 21st March, 1755.*

I RECEIVED your obliging favour of the 6th Inst. of which I am highly sensible; and shall always be subservient to your commands.

You do me the honour, to tell me, that the Royal Society desire to know the experiments, by which I found all ascending vapours and exhalations to be electrified. At first I only supposed that they must be so, according to the reasoning in my letter; but, upon trial, with a very simple apparatus, I had the pleasure to convince myself that they were so.

I extended a fine string of silk eight feet horizontally; and from the middle suspended two pieces of such down as grows upon our turf-bogs, by two pieces of fine silk, about twelve inches each in length; and then, by rubbing a piece of sealing-wax on my waistcoat over my side, I electrified the pieces of down; and then brought sundry burning things under them, so as to let the smoke pass in great plenty through and about them, to try whether the electrical fluid would run off with the smoke; but had the pleasure to see that the down was but a little affected by the passage of the smoke, and still remained electrified. I then brought sundry steams from the spout of a boiling teakettle,

kettle, and otherwise in the same manner; and still found the down remained electrified. I then breathed on them in great plenty, but found that the down still remained electrified. I then joined the palms of my hands together, with the fingers extended perpendicularly under the down, which still remained electrified; although the subtile effluvia, thrown off by perspiration, passed in great plenty through the down; as may appear by holding one or both the hands in the same manner under any light matter floating in the air, which will be driven upwards thereby, with as great velocity, as an electrified feather is by an electrified body held under it. In short, I tried all the vapour and exhalation I could think of, in the same manner, and with the same success.

I then warmed a wine glass, and, with the skirt of my coat, held inside and outside the glass between my fingers and my thumb, I rubbed the glass briskly about, and electrified the down, and found all experiments answer in the same manner as they did with the wax. I mention this particular, because some writers on electricity have said that there were two kinds of electrical fire, the one resinous, and the other vitreous; because light bodies electrified by glass, are attracted by electrified wax, &c. and those electrified by resins, are attracted by glass. But I think these different effects must arise from some differing qualities in the resin and glass, which have power to actuate

actuate this fire differently. For if there were really two distinct species of this fire opposite in their nature, the afore-mentioned experiments would have a very different consequence from what appears. For if the vapours were impregnated by the vitreous fire, they must absorb, or some way disturb the resinous fire which electrifies the down; and so, *vice versa*; but we find that the same vapour, with its electric fire, passes through the electrified down in the same manner, whether it be electrified by glass, or resin. But I will not detain you on this subject.

The electricity remaining in the electrified down after these experiments, made it appear that the smoke and steams must be either electrics, or non-electrics electrified. It was easy to suppose them non-electrics; as they arise from non-electric bodies; and the more, because the highest electrics, by a discontinuity and comminution of their parts (long before they come to be as minute as the particles of ascending vapours) become non-electrics, or conductors of electricity. For glass, resin, wax, &c. all become non-electrics, even in fusion.

But, to try whether the steams, &c. were non-electrics, I only bedewed the wax and glass with my breath, steams, &c. from my hand to the end of the wax and glass; and then touching the electrified down with the ends of the wax and glass, I found that the electrical

electrical fire immediately passed from the down into my hand through the steams, &c. which rested upon the wax and glass; which I think sufficiently proves the steams, &c. to be non-electric; and I think it as plainly appears, that they are electrified while ascending, because the electrical fire in the down, does not join with them in their passage through it; which otherwise it would do with them or any non-electric not electrified.

I made some other experiments to this purpose, but these principally persuaded me to say that all ascending vapours and exhalations were electrified; but how far they will weigh with the Royal Society, I must leave to their better judgment; and am,

S I R,

To the Rev. Mr.
Th. Birch, Secretary to the Royal Society, in Crane-court, Fleet-street, London.

Your most humble, and

most obedient servant,

HEN. EELES.

I electrified the down after each experiment.

LET-

L E T T E R V.

S I R, *Lismore, 12th April, 1756.*

I Wrote to you 21st *March*, 1755, with some electrical experiments, and therein mentioned, that some writers were of opinion that there were two kinds of electrical fire, the one resinous, and the other vitreous; because light bodies electrified by glass are attracted by the electrified wax, &c. and those electrified by resins are attracted by glass. But that I thought these different effects must arise from some differing qualities in the resin and glass, which have power to actuate this fire differently. I have often since been concerned that I did not communicate an experiment that I had then, and many times before made, the effects of which, and many others I have since made, some few of which I shall mention, may let us farther into this enquiry, and give us a new doctrine of electricity.

It may appear something late to trouble you on this subject after the labours of so many ingenious men; but as I think that there are but few things which can open a larger field for speculation to the inquisitive, than the powers of electricity in the operations of nature; I will venture to lay before you some discoveries which may be useful in investigating the powers and uses of this active principle.

I shall also boldly venture upon another beaten track, I mean magnetism, where I
think

think there is still room for enquiry, tho' numberless men, of abilities superior to mine, have gone before me ; but as truth is what we all seek, I shall not be ashamed to lay any discoveries before you which appear new to me ; in which if you find nothing useful, I hope you will forgive the trouble of reading, and dispose of them as you think proper. But I will add my opinion, that I think it one great obstruction in the way to knowledge, that from something desultory in the mind, men generally act like children in growing too soon tired of the same play-thing, and always quit these difficult researches, long before they come to the extent of the human understanding.

There are in electricity two different distinct powers, which in their natural state are always conjoined, and seem to exert but little sensible action in that state ; these powers I call, only for distinction sake, the vitreous power and the resinous power, because they seem to be differently excited by wax and glass ; not that one is excited without the other, for they are both always excited together, the one acting inward to the electric and the other out from it, as often as they are forced to separate by heat and friction ; and this order is transposed as often as the polished glass and wax are rubbed ; that power which acts out from the glass acts inward to the wax, and that power which acts out from the wax acts inward to the glass. They may be also transposed

transposed as often as you please by rubbing the same piece of glass, as I shall shew by experiment. Or you may electrify the same matter with which power you chuse, without transposing the powers, while they are excited by the wax or glass, only by drawing off that power which acts inward to, or outward from the electric. The method of doing which I shall shew presently by experiment.

Any two things electrified by any one of these powers repel each other ; but if you electrify two things, one with the one power, and the other with the other power, then these two things will attract each other ; which shews that these powers naturally attract each other, and are only separated by force. This matter being known, it will be much easier to account for the phænomena of electrical experiments than it has been hitherto.

I could send you a number of complicated experiments to prove this doctrine, and to explain the phænomena of electricity ; but I rather chuse to let it go with as plain and evident experiments as may be, lest I should err in my manner of expressing them, or the operators mistake in the executing of them, and by that means think me mistaken in what I assert ; as I well know how difficult it is, to make gentlemen recede from the opinions they have held and asserted, even by the clearest truth.

Before I mention any electrical experiments I will say something of magnetism ; as there
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are some things in the powers of magnetism mighty similar to those of electricity; but I do not by any means think them the same.

Magnetism consists of two different distinct powers, which in their natural state are conjoined and exert but very little sensible action, and strongly attract each other at all times; but when separated by force, they act similar to those of electricity; for when magnetism is excited in two different pieces of iron by the south pole, those ends repel each other, the same thing follows when they are excited by the north pole; (for so these different powers have been called from their verticity;) but if one piece be excited by the north pole, and another by the south pole, then they attract each other, as in electricity.

Both magnetism and electricity act strongest at corners, edges and points, they have their repulsive and attractive powers at certain distances; the electrified body is as much attracted by the body which is not electrified, as the latter is by the former; the magnet is also as much attracted by the iron, as the iron is attracted by the magnet. But here I must remark that the magnet attracts and is attracted according to the quantity of iron to be attracted, and not according to the strength of the magnet. For one ounce of lead at the end of a small bit of iron, will pull it off from a magnet, which will suspend a pound of lead at the end of a piece of iron weighing four or five ounces; tho' the ends of both pieces of iron, which are to come in contact with the

magnet, be shaped alike ; the cause of which I shall endeavour to explain. This has something similar to the action of gravity.

Both powers of magnetism and electricity are excited and separated by friction ; which is a very wonderful effect if duly considered in both ; particularly in magnetism, that these powers naturally attracting each other should remain separate in the same steel bar for many years ; and yet at any time be drawn back into their natural state by a few rubs of two other magnets in a contrary order to that which separated them at first ; I mean by drawing their poles from the center of the bar over the ends, the north pole over the north pole, and the south pole over the south pole, by which means the separated powers are conjoined, and seem to vanish, at least to be quiescent ; not that they really are so, for they are always set in action by a foreign power according to the position they lie in ; as I shall shew presently. The powers of electricity will also remain separate on a rubbed glass, for many hours in a dry air and warm place.

There is another remarkable similarity between these powers, for I can give a verticity to a fine needle by the electric powers, and make its poles what points of the compass I please to make choice of ; but this verticity is very weak and easily altered, and at best not permanent for many hours ; and therefore I will not trouble you with the manner of giving

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magnet

ing it. *Mr. Franklin* says in his letter, in the 47th vol. of *Philos. Transactions*, Number 44, page 289, that he has frequently given a polarity to needles, and reversed it at pleasure by an electric shock. Which I believe is true; but he does not seem to know that the electric fire, in this case, does no more than any common fire will do; as I shall shew presently.

Magnetism is a quality inherent in all iron, nor do I think that iron can be divested of it. Fire, which is the readiest means to divest iron of any quantity of fixed magnetism, does not divest it of its natural quantity, but on the contrary will fix a polarity in any iron, according to the manner of heating or cooling that iron.

Mr. Robert Boyle long since told the world that iron, heated red hot and cooled in a vertical position, acquired a fixed polarity, the under end being the north, and the upper end the south pole. He also told them that a bar of iron held vertically, had a polarity in that position, in the manner above, but that the poles were reversed upon reversing the bar. He has also told them how to change the poles of a magnet by fire, and likewise how to divest a bar of steel of its fixed magnetism, by a contrary touching; which *Mr. Canton* seems to make a secret.

Tho' he and others, have told us many things too prolix to repeat, yet I think something may be added to shew by what fixed rule magnetism is acted on by fire, as also to shew

how it acts in its natural state ; and therefore I shall add this rule ; That all bars of iron, which have not a fixed or unchangeable polarity, acquire their strongest polarity while held parallel to the dipping needle ; and that polarity lessens by degrees, as they recline from that position, (whether in or out of the magnetic meridian) 'till they come to be perpendicular to the dipping needle ; in which position the magnetism does not seem to act. And this may be proved, by touching a fine sewing needle with the magnet, and floating it upon a glass full of very clean water : for, upon bringing an iron bar in the above position, the north end of the needle will be repelled by the under end of the bar, and attracted by the upper end of the bar ; and so, *vice versa*, of the south end of the needle. And, as you recline the iron bar from being parallel to the dipping needle, you'll find the polarity lessen by degrees, 'till it becomes perpendicular to the dipping needle ; and then all signs of polarity vanish. What I have now said, will also serve for a rule for fixing a polarity in bars of iron, or steel, by fire ; for if they are heated red hot, and cooled in any of the above positions, they will have their polarity fixed according to the position ; the strongest, while parallel to the dipping needle, and so lessen by degrees, 'till their position becomes perpendicular to the dipping needle, and then they will not have any fixed polarity. And this rule holds in-
variable,

variable, provided that the iron and steel be equally heated, and equally cooled, whether in the air or in water. But I have found, upon cooling in water, that when the under end of the bar was exceedingly hotter than the upper, and that the upper end cooled first, that it sometimes became the north pole; but this very seldom happened. After heating bars of steel very hot, and plunging them into cold water in a position parallel to the dipping needle, I have found that they had not only acquired a strong fixed verticity, but also an attractive power.

I shall now say something of communicating this power. First, that large surfaces easily communicate it to small ones (but not *vice versa*) and that in a much greater degree than any they exert themselves: For, take a large bar of iron, or steel, which is better, which has not a fixed verticity, and hold it parallel to the dipping needle; then take small bars of steel, about three inches long, a quarter broad, and one sixteenth thick, and touch them to the ends of the large bar, as you would to any magnet, by drawing them from the middle of the small bar over the ends both of the small and large bar, ten or a dozen times with each side of the small bar, and you'll find the small bar endued with a strong fixed verticity and attractive power, much greater than what the large bar exerts in any position; and changed into a real magnet. And thus, by touching a number of the small bars, and

proceeding in Mr. *Canton's* method, the magnetic virtue may be raised to as great a degree as the metal can receive. For here I must observe that a piece of steel well hardened may be made to take up thirty times the weight that it will do when quite soft, tho' you touch it with the same magnetic bars in each state. And this method comes nearer to what Mr. *Canton* proposed, *viz.* to make artificial magnets without the help of natural ones. But neither will do that; for his poker and tongs are natural magnets and have their verticity fixed by being heated and then cooled in a vertical position; and the iron or steel bar, tho' without a fixt verticity, yet while it remains in that position exerts a verticity, and is able to communicate a fixed verticity to the small bar, and is therefore for the time a natural magnet. Nor is there any iron bar, from the largest to a six-penny nail, but what will exert this power according to the afore-mentioned rules; nor can they be divested of it. But how this power is raised to degrees exceedingly above that which gave it, and communicated to any quantity of steel we please in the same or greater degrees, has been matter of wonder and surprise; nor has it been more easy to account for its being so quickly withdrawn from a magnetic bar, by a friction contrary to that which gave it.

But I shall now endeavour to explain this matter. The magnetic powers in the large steel bar, while it is held parallel to the dipping needle, are acted upon by that foreign

power

power which directs all magnetism, one of them being attracted to it, and the other repelled from it, as may appear by the bars exerting a verticity in this position; and if the bar continues for a great length of time in this position, this action produces a fixed verticity in the bar by separating its powers. And while these powers are any way separated in the large bar, if a very small bar, be drawn over the end of the large bar, then that power predominating at the end of the large bar, attracts one of the powers of the small bar to that end, which was in conjunction with the large bar, and consequently forces the other power to the other end of the small bar, and by a frequent repetition of this operation the powers in the small bar are intirely separated, and a fixed verticity produced. Now when a number of these small bars have the powers thus separated, they may be joined in Mr. Canton's method to separate the powers in a larger bar, whose surface should not exceed the surfaces of all the small bars taken together; and thus you may proceed to separate the powers in what quantity of steel you please. How they may be again conjoined in their natural state, I have before shewn. Why the large piece of iron is more attracted by a magnet than the small piece, appears from there being a much greater quantity of this power to be attracted in the large piece than in the small piece.

I could produce many experiments to shew, that these powers are always separated in any piece of iron while in contact with the magnet, and that they remain so for some little

time after the magnet is withdrawn, which duration is in proportion to the size and strength of the magnet and the quantity of iron; but this would make me run into an unnecessary prolixity, because every experiment-maker may observe it. And indeed I would trouble you with no more of this kind than is absolutely necessary; but upon recollection that I have lately read a treatise upon attraction and repulsion by a member of the society, before whom it is possible you may lay this letter, whose principal design is more particularly to explain the phænomena of magnetism, I must beg leave to observe upon what he has said; because it differs very much from what I think, and what I have now said of it.

This learned gentleman says that magnetism is a continued stream of repellent fluid, circulating through the axis of the magnet, by entering at one end and going out at the other, and returning over the surface to enter again and continue the circulation. To prove this and make it evident to our sense, he brings an experiment with the filings of steel sprinkled on a sheet of paper, and laid over the magnet to shew, by the figure in which they are ranged, the direction of the magnetic stream; and to make this more evident, he chuses to make use of steel bars made magnetical, because magnetism is found more regular in them than in the natural magnet. This experiment is very easily made by sifting some steel filings, through a paper pricked with a pin, very thin upon a sheet of paper laid over the magnetic

tic bar, and putting the paper gently round the edges with the end of a quill, the filings are immediately ranged in order by the magnetic powers. But this order will never shew that they were thrown into it, by a stream of repellent matter, circulating through the axis of the magnet, from north to south, and back again over the surface from south to north; nor is there any thing in this order similar to what this gentleman has described and delineated in his printed book. Therefore as the existence of such a stream is not proved by this experiment, I shall not meddle with this gentleman's abstruse reasonings about it.

But as this experiment will help to explain what I have said about the magnetic powers, I shall mention the order in which the filings are ranged by a single magnet, and in some other complicated cases when more magnets are used. And if I should fail in describing, the experiment will make it explicit. I have said before that the magnetic powers are separated in every bar rendered magnetical, and consequently must be drawn toward the extremities of that bar; the point of greatest power in either end of the bar is easily discoverable by suspending a very fine needle, touched by that end of the bar, and bringing it over the axis of the bar, it will be drawn to that point; which is near a semidiameter of the bar, within the end of the bar; this power also extends, in the axis of the bar, toward the middle of the bar, lessening by degrees. Now let us suppose this power in form something

something like an egg, but longer and tapered toward the end next to the middle of the bar, with its rays thrown out every way perpendicular to its surface; this will nearly describe the order in which the steel filings are always ranged in this experiment; with this exception, that where the rays from the inward ends of the different powers should cross each other near the middle of the bar, they are there, by their mutual attraction, drawn into curved lines; which are more curved in proportion to their distance from the bar, as the angles in which they should have crossed, are less obtuse. These lines may make an hasty observer, think that there is a kind of circulation about the middle of the bar, and he may be more confirmed in that opinion by the suspended needle, I just now mentioned, which will be thrown off from one side of the bar and drawn round to the other, in the direction of these lines; but let another needle be touched by a different end of the bar, and suspended in like manner, it will be carried back over the same lines in a direction contrary to the first. Now if these needles are carried by the force of a circulating fluid, there must be two fluids circulating in direct opposition to each other; and it is very difficult to conceive how these needles can always keep the same different courses, without ever being interrupted by the fluid moving in opposition thereto.

But lay two magnetic bars, parallel to each other, and about two inches asunder, the north

north pole of one and the south pole of the other, at each end, and lift the filings on the paper over them, and you have a new set of circulations produced, if you suppose that the fluid circulates in the lines in which the filings are ranged; for here you have curved lines wherever the attracting rays should have crossed each other, both at the ends and about the middles, as in the former experiment, and from side to side reversed to those in the middle. But not to trouble you with a number of experiments which are easier made than described, I shall only say in general, that place the bars in what complicated shapes you please, the filings will always range themselves according to the attraction or repulsion of the powers in the bars, and that in such shapes, as no ways indicate a circulation of the magnetic power through the axis of the bar, and back again over the surface. Nor can I think there is any circulation between these different powers; for if there was, they must mix by their natural attraction, and then magnetism would seem to cease, as it does when these powers are drawn together by rubbing them with other magnets in an order contrary to that which first separated these powers in the bar. And to shew that these powers are really separated in every magnetic bar, suspend a touched needle by the eye and point, so that it may hang parallel to the bar lying on the table, carry this needle round the end and over the middle of the bar, you will find the north end of the
needle

needle attracted to the south end of the bar quite round, and the south end of the needle thrown off every way in the direction of the rays which I have described ; then bring the south end of the needle to the north end of the bar, and it shall be attracted quite round, and the north end of the needle thrown off in the direction of the rays. When the repelling ends of two magnetic bars are laid together, the needle will be carried round them and over the middle of the bars in the same order it is carried round one end of the bar. If you place two bars in a line with the north end of one to the south end of the other, separated to one third of the length of the bar (to which distance the powers seem to be separated in most bars) and then sift the filings over them, you will find them ranged between the bars, in the same manner they are ranged about the middle of each bar. Which shews that these powers, separated to the same distance in the ends of two bars, act much in the same manner as when separated in the same bar ; except that they are weaker in their ends approaching the middle of the bar, as I have observed before.

I could pester you with experiments with file dust, suspended needles, &c. but as magnetic bars are so common, I shall leave every gentleman to make experiments for himself, which will in the end (as far as I know) evince the truth of what I have said ; for I fear that I am already growing too prolix, which with many other faults, I must beg you will excuse,

cuse, as well as the want of proper deference and respect, due to you and the society, in my manner of writing ; for if I conceived I was in presence of so learned a body of men, I should be easily deterred from writing or speaking ; but the discoveries I am now laying before you are my own, and appear to me, by a thousand experiments, to be founded in truth ; and if no one else has hit upon the same things, they may be worth the acceptance, as they tend to explain the phænomena of electricity and magnetism ; and furnish a new doctrine of both ; which if you please may be called *Eeles's* new doctrine of electricity and magnetism ; not that I intend to include in this letter all that I know of these powers, for it may appear absurd to speak of their different actions according to their different circumstances, or of their uses in nature, before it is allowed that there are such different powers.

I shall now mention my electrical experiment, and some few others, to explain what I have said.

The experiment is this. Extend a silken string horizontally, and from it suspend two pieces of bog-down, by two silken strings, of fifteen inches long, looped on the horizontal string to slide backward or forward ; then electrify the pieces of down, one with sealing-wax and the other with glass ; and when you bring them to approach within the sphere of their activity, they will fly together, and upon contact all signs of electricity vanish ; provided that the pieces of down were equally electrified

fied, whether much or little; but if one was much more electrified than the other, then that which was least electrified loses all its electricity after contact, and the two remain electrified with the excess of electricity of that which was most electrified, which will appear from their being repelled by the wax or glass, which ever was used with the piece which was most electrified.

This experiment and many others led me to think that there must be two different powers in electricity, and that they must be differently excited by the wax and glass; accordingly I observed, that while a piece of down, such as I used, was electrifying by wax or glass, that some of its fibres were violently extended from the electric, while others were as strongly attracted to it; this made me think that the powers may be here divided; and that if I could draw off one power the down may remain electrified with the other; I then kept my finger in contact with the outward fibres, for a second or two; and then plucked away glass and finger together, and had the pleasure to find the down was electrified by the resinous power, as appeared by its being attracted by the glass, and repelled by the wax. I then electrified the down in the same manner with the wax, and found that it remained electrified by the vitreous power, and was accordingly attracted by the wax, and repelled by the glass. This convinced me that my supposition was true. I then put the down toward the bottom of an excited wine glass, which

which was taper, so that the down was in contact with all sides of the glass ; and when it had remained there for a second or two, I plucked the glass perpendicularly down from it, that it might not touch the mouth of the glass, and found that the down was electrified with the resinous power. This I thought put the matter out of doubt. I then electrified the two pieces of down with the same glass ; but one of them in the common way, with the vitreous power which acted out from the glass, and the other by the resinous power which acted inward to the glass, by applying my finger as afore-mentioned, and found that they attracted each other in the same manner, which they did when electrified by the wax, and glass ; and that after contact the electricity vanished, provided they were equally electrified ; but if one was more electrified than the other, then they both remained electrified with the excess of electricity in that which was most electrified ; as may appear by their being both either attracted or repelled by the glass. And this I can always order as I please in the experiment. I made the same experiment with excited wax, and the effects were the same. I then suspended a piece of down, from the silk string, by a piece of linen thread about twelve inches long, and found that it could not be electrified with the vitreous power by the glass, nor with the resinous power by the wax ; the reason is because there was more of that power, which acted inward to the electric, in the down, than

than there was of that power which acted outward in the thread. But reverse this experiment by letting a piece of linen thread hang from one of the pieces of down which is suspended by silk, and then touch the end of the thread with the glass or wax, and the down is immediately electrified; because there is a great deal more of the power, which acts outward to the down and thread, than there is of the power, which acts inward, in that part of the thread which is in contact with or near the glass.

These experiments and many others made me think it may be possible to make glass, by altering its surface, transpose these powers in the same manner that wax does. In order to which I took some coarse emery, and obscured one side of a pane sash glass, which, when finished, I excited by rubbing that side of the glass, and found the powers transposed at both sides of the glass, in the same order as they are by excited wax; the resinous power outward and the vitreous power inward. And they are again transposed into the different order at both sides of the glass, when you rub the smooth side of the glass; and so, *vice versa*, as often as you rub the different sides of the glass. Which may be seen by rubbing the smooth side of the glass and electrifying a piece of down, which will be repelled by either side of the glass; then rub the obscure side of the glass, and the down will be attracted by either side of the glass; rub the

the smooth side again, and the down will again be repelled by both sides of the glass; and soon, attracted and repelled as often as you rub the different sides of the glass, and three or four rubs at a side are sufficient to transpose these powers. Or if you rub one half of the breadth of the glass at one side, and the other half at the other side, the piece of down shall be attracted at each side of one half of the glass, and repelled by each side of the other half of the glass. In these attractions and repulsions the down must not be suffered to touch the glass. But if it should touch one half, and be electrified and repelled, then the other half will attract it.

When the obscure side of the glass is rubbed, let two pieces of down hang near each other, and bring the glass under them, they will both be strongly attracted to the glass, but some of their fibres extended from it; draw off one of them in such manner that it may touch the upper fibres of the other in going off, and immediately after draw the glass from the other, and you will find these pieces of down electrified with the different powers; that which was first taken away by the power acting outward, and the other by the power acting inward. Note, in this experiment the down must not touch the glass, if it be within half an inch, it is sufficient; and the same in other experiments where the outward power is to be drawn off by the finger or other non-electric. Sometimes when the down is very much attracted to and re-

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pelled from the glass, in its different fibres, if you withdraw the glass suddenly, there will not be the least appearance of electricity in the down; because the outward fibres were electrified by the power acting outward, and the inward fibres by the power acting inward, and these powers joining by their natural attraction after removing the glass, they are in their natural conjoined state, in which they exert but little sensible action.

But if any one doubts whether these powers are really separated in the down, while it stands thus attracted to, and repelled from the glass; let him bring a piece of excited wax to the repelled fibres, and they shall be attracted to it, and it will repel the fibres which are attracted to the glass; or let him bring another excited glass, and the upper or repelled fibres shall be repelled by it, and the lower fibres attracted to it.

The same effects will appear, in a contrary order, when the down stands so excited upon a piece of wax.

I may now appear tedious, but I have taken some pains to reduce my letter to these simple experiments to prevent objections to it; and I believe that I need not inform you that these discoveries were not made merely by the touch of a finger as I have described; for inventions cost the inventors much more trouble, than can easily be believed by those not used to invention, and who only see the thing when done.

I could

I could now enter upon a number of complicated experiments, to shew how the natural quantity of electricity, in all bodies, must be acted upon to admit the adventitious, and various other things, relative to the complication of these powers. And also in magnetism, particularly to shew how the file-dust may be disposed in curve lines without any circulation between the magnetic powers; but I shall reserve these matters for some future amusement.

About two or three years ago I wrote something to shew the cause of the refraction, &c. of light, according to my promise to the society, to which I had added some answers to Sir *Isaac Newton's* queries; but upon meeting an advertisement in the forty-seventh vol. of *Philos. Transactions*, that every man was left to his own credit, for the support of his opinions published in the *Philos. Transactions*; and my opinion in that tract being contradictory to several great men, who wrote on that subject, I rather chose to throw them by, than dispute about them; not that I think them insignificant, but I believe my credit very insufficient to bear me through such an opposition. I also wrote something to explain the cause of the *Aurora Borealis*, and other things, which I may sometime or other communicate for your amusement. I am,

S I R,

Your most humble and
most obedient servant,

HEN. EELLS.

Lest I may be suspected of boasting of a greater knowledge in electricity than I really have, and that I would impose a new doctrine from a few simple experiments ; I shall set down a few of many hints, to which I am prepared to speak, and also a few of many more complicated experiments which I have made.

Heat is as necessary as friction to excite the electrical powers to action. The electric powers must be separated in the body to be electrified, as well as on the electric. This will explain the action and power of points in electrical experiments ; they do not act beyond the electric atmosphere. The electric atmosphere of the clouds is nothing similar to what has been supposed. The electricity found in the air, only adheres to the vapours floating therein. Points of no great use in drawing off electricity from the clouds in time of thunder ; that being generally discharged by an exchange of powers from cloud to cloud, &c. sometimes from the cloud to the earth, through the first conductor. The mysteries of Mr. *Muschenbroeck's* bottle easily explained by the separation of the electric powers, and by their action when separated. These powers act freely through thin glass ; and where their passage through it is prevented, it is by one of the separated powers, and not by the glass. All the complicated phænomena in different matters hanging on, or applied to the same conductor, may be explained by the action of the

the natural quantity of electricity in the conductor, according to the application of the adventitious. Mr. *Franklin's* supposition, or rather assertion, that the electricity which passes from the inside of the electrified bottle to the outside, during the explosion, is lodged in the body of the glass, is quite wrong; for that electricity is not even in contact with the glass, while the bottle is charged; but on the contrary, a considerable part of the non-electric, which is in contact with the inside of the glass, is electrified by the opposite power of electricity.

It may appear invidious in me to object to any opinion of this ingenious gentleman, because we both happened to write hypotheses to explain the cause of thunder; which opinions are now in the hands of the public; and must stand or fall by the judgment of the ingenious inquisitive. I never heard of Mr. *Franklin's* name 'till some time after my last letter to you; I lately got and read his works, and if I wrote in a vindictive spirit, I could easily shew that his means of electrifying the clouds, his manner of bringing them into action to produce thunder, and his proposed method of drawing the electricity from the clouds by pointed iron, are all wrong. But I think the world obliged to this gentleman, and many others who have taken so much pains to investigate the nature of this subtle power. But as there is still great room for enquiry, neither Mr. *Franklin*, or any ingenious gentleman, ought to take it ill, that

their errors are discovered ; because authorifed errors are the greateft bar to knowledge ; witnefs the great deference paid to many of *Aristotle's* abfurdities to this day ; nor ought any gentleman to think of impofing on himfelf or others, for the vanity of fupporting an opinion. And I folemnly declare, that if any of my opinions fall under a juft cenfure, I fhall be much obliged to him who fets me right. I have faid fo much, becaufe I think that what Mr. *Franklin* has produced in the 49th vol. of *Philof. Tranfactions*, page 300, as principles, will be found fo far from being principles, that they are not ftrictly true.

His firft principle, that electric atmofpheres repel each other, is fo far true, that electric atmofpheres of the fame denomination repel each other ; but when they are of different denominations, they then attract each other.

His fecond principle is in part true, and may be explained by the fame means ; for if one body is more highly electrified than another body is by the fame power, when the firft approaches the fecond, the exterior electricity of the firft repels the exterior electricity of the fecond, but, *per contra*, attracts the interior electricity of the fecond ; which, if duly confidered, will explain the effects of Mr. *Franklin's* experiments.

As to his third principle, that bodies electrified negatively repel each other, it is erroneous ; for no body can be electrified negatively, as may appear from what I have faid
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in my letter, and shall say presently ; for the power greatest in quantity always acts outward, and the body is electrified by that power.

Nobody will appear to be electrified while the electric powers are equal in or on that body ; but if there be a greater quantity of one power than there is of the other power, then shall the greatest quantity act outward from that body, and the body will be electrified with that power, and repel another body electrified in the same manner by the same power ; but if the second body be electrified by the other power, then shall they attract each other, and, after contact, all signs of electricity vanish, if they were equally electrified ; but if not, both will remain electrified with the excess of electricity of that body, which was most electrified. All this appears in my first electrical experiment ; but as the explanation of all electrical experiments depends upon rightly understanding this proposition, that there are two different powers in electricity, and that they act in this manner, I would willingly be fully understood.

The same conductor may be first electrified by either of the electric powers, by an excited glass, and then by the other power, without altering any circumstance in the conductor or glass, but merely by the manner of applying the glass to the conductor. The same piece of down may be electrified by an excited glass, so as to be attracted and repelled many times, as you think proper by the

glass, without any fresh rubbing of the glass, or any other additional matter touching the down. The same conductor may be electrified with either or both the different powers in succession by the excited glass, while in contact with the conductor, and all signs of electricity be withdrawn with the glass, without any other matter touching the glass or conductor. Two small conductors, with a piece of down hanging on each conductor, may be so suspended, that with one touch of an excited glass, one conductor shall be electrified with the vitreous power, and the other conductor with the resinous power, and yet both pieces of down shall be electrified with one and the same power; and upon withdrawing the glass, all signs of electricity in the pieces of down, and in the conductors, shall disappear; or by the manner of withdrawing the glass, you may leave both conductors and both pieces of down electrified with one and the same power.

A small conductor, with a piece of down hanging from it by a linen thread, may be electrified through the down by one side of a pane of sash glass, which is excited by rubbing the other side of the glass; and yet by continuing to rub the same side of the glass, all the electricity may be drawn back again through the glass from the conductor; and farther yet by continuing to rub the same side of the glass in the same manner, the down and conductor may be electrified with the

the power which is different from that which electrified them at first.

I could easily tire you with a recital of experiments to evince the truth of what I have said ; but much more so, were I to expatiate on the use of this combination of powers in the reflection and refraction of light, in the secondary attraction and repulsion in respiration and perspiration, in evaporation and exhalation, in sensation, in vegetation, &c. but even the catalogue is too prolix to repeat, and lays open a train of enquiry too extensive for my abilities, or inclinations to pursue ; for to say truth, I have been more addicted to the pursuit of what the world calls pleasures, than to philosophical researches ; however if what I write is any manner of entertainment to the Royal Society, or to you, I shall do myself the honour, at times, to continue a correspondence.

To the Reverend Thomas
Birch, Secretary to the
Royal Society.

* * This letter was sent, dated the 9th
of *August*, 1757.

L E T.

L E T T E R VI.

S I R,

Lismore, 14th Jan. 1758.

I WROTE to you on the 9th of *August* last, with a new doctrine of electricity and magnetism, but as I have not been favoured with an answer, I suppose that I have not made my doctrine intelligible; and indeed I am conscious, from the scattered manner in which I have delivered my thoughts, and from not taking my experiments from the apparatus which is commonly used, that method, and a great deal more, is wanting to explain those subjects fully. As to the want of method, I am only guilty of a general neglect; for I think that no one has yet attempted to give a theory from facts to explain the first action by which electricity is made sensible to us; I mean the attracting and then repelling any light matter, which is placed within the sphere of activity, of the excited electric. If this method had been thought of, it would have led on to a more easy explanation of the effects of more complicated experiments. But I observe that a great deal of embarrassment has arisen from the adopting a few terms, which are unintelligible; such as bodies being electrified negatively, and that glass throws out the electric fire, and that sulphur and resins drink it in; for how is it possible to conceive

ceive that two bodies shall repel each other the stronger, the more the repelling power is taken from them? or how can we suppose that glass, is always throwing out the electric power, and sulphur always drinking it in, without one being ever full or the other empty? I think the adopting these terms has occasioned the loss of a great deal of refined reasoning; but that I may not speak too presumptuously, I shall proceed to deliver, in part, what I have to say of electricity.

Electricity consists of two different distinct powers, which always strongly attract, and condense, each other, and therefore when they are in equilibrio, they exert but little sensible action; but when the equilibrium is destroyed by a diminution of one, and the encrease of the other, then the encreased power has a power of expansion, in proportion to the subtraction of the other attracting power; by which means it forms itself into what we call an electric atmosphere; which atmosphere will repel any other atmosphere raised from the same power, but attract any atmosphere raised from the other power of electricity. These atmospheres are always formed with an increment of rarity, as they recede from the lessened attractive power, which lessened power always acts inward to the body which is electrified. These powers I have before called, only for distinction sake, the vitreous and the resinous powers. These powers are strongly attracted by all matter, and are always found adhesive to all matter,

ter, pretty near in proportion to the specific gravity of the matter, as far as I have been able to judge. Nor is it easy to subtract any part of either of these powers from any part of matter, without adding an equal portion of the other power, which addition it will retain, until it is supplied with an equal quantity of the power first subtracted. So that when a spark is taken from the conductor, or any body electrified by excited glass, there is as much of the resinous power passes from the finger into the conductor, as there is of the vitreous passing from the conductor into the finger ; but of this in the experiments. I hinted in my letter that the electric powers must be separated in the body to be electrified, as well as on the excited electric. That they are so, even before they make any exchange of power with the electrified body, will appear by bringing any body, without points and suspended by electrics, into the atmosphere of the conductor, but not so near as to snap, suppose within two or three inches, you will find the end next the conductor electrified with the resinous power ; and the end farthest from it electrified with the vitreous power ; which may be tried by suspending a piece of down by silk, and touching the down to one end, it will be electrified with the vitreous power, and will be attracted by the wax, and repelled by the glass ; and upon touching it to the other end, it will be electrified by the resinous power, and be repelled by the wax, and attracted by the
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the glass. That this body is thus electrified by the electric atmosphere of the conductor, by attracting one of the powers, naturally adhering to that body, and by repelling the other power, without making any exchange with the electricity of the conductor, may be thus proved. Bring the same body, divested of its electricity, (or, more properly speaking, with its electric powers brought into an equilibrium;) by touching it with your hand, or some non-electric, within the same distance of the conductor as before, and consequently the different ends must be electrified with the different powers as at first, while it remains in that situation; but remove it from the atmosphere of the conductor, and bring the suspended down to touch it; you will find no signs of electricity in the body; the powers then only acting upon each other are drawn into their natural condensed state all over the body, in which state they exert but little sensible action.

This experiment may shew how the file dust, sprinkled on a paper over the magnet, comes to be ranged in curved lines, where the magnetic atmospheres attract each other into that form; for every grain of dust has its magnetic powers separated by the magnetic atmosphere, and their poles ranged in the same direction with the magnetic atmosphere, consequently they must attract each other, while they are magnetical, into the same direction with the magnetic atmosphere; and this may be, and is done without any mixture

mixture of the powers in magnetism, as it is in electricity; as I can prove by experiments. But to return and leave magnetism for some future amusement.

I must here beg your patience while I add two or three more experiments, to shew that the natural quantity of the electric powers in all bodies is thus acted upon by an electric atmosphere, while any body is immersed in it. For much depends on rightly understanding this matter.

Take a strong wire ten inches long, turn the ends into small loops, fix the middle to a glass handle with sealing wax; from each loop suspend a piece of down by a linen thread six or seven inches long; bring the handle perpendicular over the conductor, the wire at right angles with the conductor, and parallel to the horizon; as you lower your hand you will see the down strongly attracted to the conductor, by the resinous power being drawn into the down; and the vitreous power repelled into the wire; but when you approach the wire so near as to change powers with the conductor, the down flies off to each side. Divest the wire and down of the adventitious electricity, and bring it as before but the wire parallel to the conductor, when the down comes within two or three inches of the conductor, it will be strongly attracted to the conductor, and by keeping your hand at the same distance and the wire parallel to the conductor, you may carry it quite round the conductor with the down attracted in all positions.

ons. But sink your hand quite below the atmosphere of the conductor, and then raise it with the wire parallel to the conductor, the wire will first approach the conductor, and therefore the resinous power will be drawn into the wire, and the vitreous power repelled into the down, consequently both pieces of down will be repelled by the conductor. Bring the wire endways toward the conductor, three or four inches above the horizontal level of the conductor, and you will see the piece of down next to the conductor, attracted to it, and the other repelled from it; the one being electrified with the vitreous power, and the other with the resinous power; which may be proved by bringing a piece of excited sealing wax, near either of them; but turn the handle that the wire may change end for end, and the powers will be immediately changed in the pieces of down; that next the conductor being always electrified by the resinous power. But while the pieces of down remain one attracted and the other repelled, take a piece of wire in your other hand, and touch the wire supporting the down, you will see the fibres of the repelled down immediately drop and rise again, the down will then be attracted to the conductor, and be found to be electrified with the resinous power. The reason of this change is, that all the vitreous power in the wire and down is repelled into your arm by the atmosphere of the conductor, and an equal quantity of the resinous power

drawn
and repelled each other
attracted

drawn from you into the wire and down. Suspend a small cork ball with silk, that it may hang upon a level with the conductor, and within five or six inches of the conductor, it will remain quiet there; but if you touch it with the end of a wire held in your hand, the vitreous power of the ball is driven into you, and an equal quantity of the resinous power carried to the ball, by which means the ball is strongly electrified with the resinous power, and is attracted with great rapidity to the conductor; and if your wire be any thing pointed and directed against the ball, the stream of resinous power flowing from it, will keep the ball fixed to the conductor, by continually supplying it with the resinous power; but when you remove the wire, the ball then parts with its resinous power to the conductor, in exchange for the vitreous power, of which the conductor has the greatest quantity, and the ball becomes electrified with the vitreous power, equally as the conductor is electrified, and is then consequently repelled from the denser part of the atmosphere of the conductor to the rarer, the ball carrying its own vitreous atmosphere with it. That all electric atmospheres, whether vitreous or resinous, are rarer as they recede from the electrified body I shall prove presently by experiment. Suspend two cork balls, by linen thread, over the conductor, and reaching within two or three inches of the conductor; you will find them electrified by the resinous power and repelling each other; but
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attracted to the conductor; for the vitreous power is driven from, and the resinous power attracted into them, by the vitreous atmosphere of the conductor while the wheel is turning; but stop the wheel and touch the conductor, and the balls fall together in their natural state. Try the same experiment with small balls suspended by silk lines of six or eight inches long, the rest of wire; you will find the balls electrified with the resinous power, after some time turning the wheel; stop the wheel and touch the conductor, the balls will still remain electrified; because it required time and force to drive off the vitreous power through the silk, and to attract the resinous power through the same; but when the atmosphere of the conductor is removed, there is not any power to drive back the vitreous power through the silk to the balls. For here I must observe that silk, wax, glass and all electrics, have their powers separated, and are electrified with the different powers, which are inherent in themselves, at their different ends, while suspended in an electric atmosphere, in the same manner that non-electrics are; but not so suddenly; as their powers will not change place so quick; and in this slowness of changing consists the difference of electrics and non-electrics. Suspend a glass by clean silk over the conductor at three or four inches distance, and after some time turning the wheel, you will find the end next to the conductor electrified with the resinous power, and the other end with

the vitreous power ; as will appear by touching each end with a piece of down, suspended by clean silk. The same effects will appear in wax in the same circumstances ; but, more evidently if the conductor is electrified by a wax globe ; but then the powers will change place, the vitreous power will appear next to the resinous atmosphere of the conductor, and so in all other experiments, when a wax globe is made use of. But I have all along supposed a glass globe to electrify the conductor. Glass and wax suspended by silk may be electrified, as any non-electric is, by a wire communication from the conductor, which must remain for some time, and then be removed by a wax or glass handle.

From the different powers existing, and being capable of changing place in all electrics according to the power applied, you see the necessity of observing whether you may not be deceived by it, in your experiments. You also see the necessity of clean silk, wax and glass, otherwise a man may be deceived in many experiments ; glass in particular, if it is so cold as to attract the damp of the air, is very apt to deceive. Let a piece of down hang by silk within four or five inches over the conductor, then with a wire, with a wax handle, electrify the down from the conductor, it will be repelled ; then touch it with the end of a wire held in your hand, it will be attracted towards the conductor, as you pursue it to touch it, it will be highly electrified with the resinous power, and strongly
attracted

attracted to the conductor, but repelled from your wire; the change of powers in the down must certainly be made through the wire, to and from your arm, the vitreous being driven into and the resinous attracted from it, by the power of the vitreous atmosphere in which your arm is immersed; and the down will retain its resinous atmosphere when carried quite away from the atmosphere of the conductor. Tho' I fear I am growing tedious, yet I will add one experiment more, which I take to be an *Experimentum Crucis* in this case, which cannot be too much explained.

Take a cylinder of lead five, six or seven inches long, more than half an inch diameter, round off the ends smoothly, then saw it through the middle and bore a gimlet hole in each of the sawed ends, fit a peg of dry wood tight into one hole and so that it may pass easily into the hole of the other piece; suspend each piece by a double piece of silk, the ends of which may be fastened to the ends of two sticks three or four inches long to prevent the cylinder from twisting, &c. the silk should be fourteen inches long or more; suspend these two pieces, joined by the peg in the middle, as one cylinder, about two or three inches above the conductor, then remove them, still joined together, quite out of the atmosphere of the conductor, and bring a piece of down, suspended by silk, to touch them, and if they shew no signs of being electrified, you have held them far enough above the conductor;

conductor; then carry them back and hold them at the same distance from the conductor, and when they have remained there for a few seconds, lift up the upper piece and take it quite out of the atmosphere of the conductor, and afterwards also the under piece without letting it come nearer the conductor in the removal, you will find the upper piece electrified with the vitreous power, and the under piece electrified with the resinous power; for when a piece of down is touched to the one, it will be electrified and repelled by the wax, but when touched to the other it will be attracted by the wax; or the down will play between both 'till it restore the equilibrium, and no more signs of electricity appear; which proves that the pieces brought no electricity from the conductor, and that the powers naturally belonging to them were separated by the atmosphere of the conductor in the manner I have been all along describing. Or let two cylinders touch each other, and all signs of electricity vanish upon restoring the equilibrium between the natural powers of the cylinders. If equal, but different, powers of electricity were only intimately mixed by their mutual attraction, all signs of electricity would vanish; but I shall shew that they are really condensed thereby, and that they again expand themselves in proportion to the attraction taken off by withdrawing any part of either of the powers.

From what I have been saying, I think it easy to explain how light bodies will be attracted, from the surface of a non-electric, to the

the conductor; for when they are brought within the atmosphere of the conductor, their vitreous power and that of the body on which they rest is repelled, and the resinous power attracted, by which means the small bodies become strongly electrified with the resinous power, and are therefore attracted to the conductor, 'till they exchange their resinous power for the vitreous power of the conductor, and then they are repelled in the manner I have mentioned of the cork ball. But in other circumstances where these small bodies are placed upon a perfect electric (if such a body were to be found, which I think is not to be found) or supposed to be free from all contact with any other body, with which they might exchange the electric powers to destroy the equilibrium, and consequently where they must be attracted with their powers in equilibrio 'till they exchange powers with the conductor; the attraction is exceeding weak; and it will require a farther disquisition to explain the manner in which it is performed. From what I can gather from the actions of the electric powers it appears to me, that a vitreous atmosphere repels another vitreous atmosphere with a force exactly equal to that, with which it attracts a resinous atmosphere of equal density with the repelled vitreous atmosphere. And that the attracting and repelling atmosphere acts with a force proportional to, and encreasing with, its own density. From hence it follows that a small sphere, whose electric powers are equal, sus-
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pended by a perfect electric in the atmosphere of the conductor, must have its powers equally attracted and repelled ; but as the resinous power is attracted to, and the vitreous power is repelled from the conductor, the resinous power comes into a denser part of the atmosphere of the conductor, and the vitreous power is driven into a rarer part of the same atmosphere ; consequently the resinous power is more attracted by the denser part of the atmosphere, than the vitreous power is repelled by the rarer part of the atmosphere ; and the difference between this repulsion and attraction must be the force with which the sphere is attracted to the conductor. It is easy to see that this attraction of a sphere can be but very weak, and accordingly experiments evince the truth of this doctrine. Extend a wire horizontally across your room at right angles with your conductor, but two feet above it ; and from wire hooks, to slide forward or backward on the horizontal wire, you may suspend your balls. Suspend a cork sphere of half an inch diameter, with very clean silk, to hang upon a level with, and within three or four inches of the conductor ; turn the wheel and you shall scarcely perceive the cork to be attracted ; but when the cork is placed near enough to the conductor to be attracted, the motion begins very gently, but the velocity encreases as it moves toward the conductor, notwithstanding it is obstructed by the gravity of the cork, which moves up in the arch of a circle to which the string is radius ; but when the cork parts with its resinous power to the conductor,

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it is repelled with great rapidity ; and if it strike against your hand or any non-electric in the atmosphere of the conductor, which by that means has its resinous power drawn towards the conductor, the cork becomes electrified with the resinous power, and is attracted to the conductor, with a force exceedingly greater, than that with which it was attracted while the powers were equal in the cork. Suspend another cork ball, exactly similar to the first, by linen thread, hanging something lower than the first, and placed twenty inches or two feet from the conductor, you will find it more attracted at that distance, than the first is at four inches distance ; the reason is, because the vitreous power is repelled from the ball along the thread, and the resinous power attracted to the ball, by the vitreous atmosphere of the conductor, by which means the ball becomes electrified by the resinous power, and is more attracted by the conductor, tho' placed at such a distance from it, and in a part of its atmosphere so much rarer, than the ball suspended by silk is ; because the powers remain in equilibrio in the latter, which therefore can only be attracted by the laws I have last mentioned. Place the ball, suspended by silk, within four or five inches of the conductor, and that suspended by thread within nine or ten inches of the conductor ; then turn the wheel briskly, the ball suspended by thread will rush by that suspended by silk, and remain fixed to the conductor as long as the conductor remains electrified with any

force, and the ball on silk remains at, or very near its first distance. The ball on thread is fixed to the conductor by the continual stream of resinous power flowing in from the thread, in the place of the vitreous power carried off by the thread. Place the balls at the same distance's again, but take off the cushion from the globe or cylinder, and with a rubber in your hand press the globe lightly to raise the atmosphere gently, that the outer ball may be drawn towards the inner ball with a slow motion, and when it comes to touch it, or so near as to exchange powers with it, the ball on silk will be drawn into the conductor and fixed there by the resinous stream flowing from the ball on thread; and if the atmosphere of the conductor be neither increased or diminished, the ball on thread will remain pretty near the place where it met with the silk ball; but if the atmosphere of the conductor be much increased by rubbing the globe harder, then the ball on thread will be also attracted to the conductor, and both balls remain fixed there, provided that they lie in contact, that the ball on silk may be supplied with the resinous power flowing in by the thread; but if they do not touch, then the ball on silk flies off, and the ball on thread remains fixed to the conductor. Cork balls, no bigger than peas, brought on a large flat glass which is quite dry and cold (for if a glass be warm, the powers are much sooner separated in it than when cold) and approached quick

quick to the conductor, before the powers have time to change place in the glass, the balls will not be attracted to the conductor till almost in contact with it; but from a non-electric they will be attracted at a considerable distance. The reason why, I have given before. Take a cork ball of an inch diameter suspended by silk; and a cork cylinder four inches long, and of the same weight with the ball, the ends rounded off, and suspended by silk parallel to the horizon. Let them hang separately within an inch and half or two inches of the conductor, that the end of the cylinder and side of the ball may be at equal distance from it; you will find the cylinder drawn into the conductor when the ball is scarcely moved; notwithstanding that the center of gravity of the cylinder hangs an inch and half farther from the conductor than that of the ball. Which shews the electric powers being farther separated in the cylinder than in the ball, that the difference of attraction and repulsion of those powers by the vitreous atmosphere of the conductor is greater in the cylinder than in the ball, because they lie in greater degrees of the density and rarity of the atmosphere of the conductor, and therefore the cylinder is more attracted than the ball. I could add more experiments to this purpose, but I believe these will be thought sufficient; as I have other matters to touch on.

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Though it may appear, from what I have been saying, that electric atmospheres have an increment of rarity, as they recede from the body electrified; yet, it may not be amiss to bring an experiment or two, for farther proof. As to the ratio in which it increases I shall leave to others to amuse themselves with; but I think it will prove an endless search; as it has done in magnetism, where there must be a different rule for every difference of strength in the magnets which approach each other, or for every different degree to which the magnetic powers are separated in the same magnet; the reason for which, I may, some time or other, trouble you with. Take a needle of four or five inches in length, supported on a pivott, like a compass needle, no matter whether it is magnetical or not; place it perpendicularly over the conductor, upon a non-electric point fixed to the conductor; this needle must be electrified with the same power with which the conductor is electrified, and, consequently, their atmospheres must repel each other; and if the atmosphere of the conductor was of equal density, there would not be any reason for this needle's resting in one point more than another, after it had been whirled about; but you'll find, when it begins to stop, that it will traverse backwards and forwards, until it fixes at right angles with the conductor; the ends thereof with their electric atmospheres, being strongly repelled from
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the denser part to the rarer part of the atmosphere of the conductor. Arm the ends of this needle with two pistol balls; the effects will be the same. Or, place a magnetic needle, in the same manner, upon a gun barrel, suspended length-ways in the magnetic meridian; neither the directive power of the needle, or the attraction of its ends to the gun barrel, will prevent its being repelled from the denser to the rarer part of the electric atmosphere, and fixed at right angles with the barrel. Place a point upon a non-electric, that it may stand upon a level with the center of the conductor, and at such a distance, that the balls on the ends of the needle, when placed upon the point, may come within a quarter or half quarter of an inch of the conductor. Here the needle is differently circumstanced from what it was when placed on the conductor; for both ends are electrified with the resinous power, and, consequently, equally attracted to the conductor, while parallel to it; but when one ball begins to move, it is more attracted, by getting into a denser part of the atmosphere of the conductor, and by the other balls being less attracted in the rarer part of the atmosphere; but most of all by the resinous power being drawn into it through the point which supports it; and thus the ball gets into a considerable velocity by the time the needle comes to right angles with the conductor; and here a re-action would begin to obstruct its circulation, if it were not prevented for some little time, while the ball

ball exchanges powers with the conductor, and the conductor is again sufficiently electrified to draw an equal quantity of the resinous fire into the ball; but in this time the ball begins to recede from the conductor, with a velocity sufficient to prevent the future re-action from stopping the ball, 'till the ball at the other end of the needle becomes nearest to the conductor, and, consequently, must be acted on as the first ball was. And thus, by an encrease of velocity in each turn, the needle soon comes to run an hundred ~~times~~^{turns} in a minute. It is indifferent which way the needle begins to turn. But this needle will not circulate, placed upon a point in a dry glass stand; nor will it circulate at such a distance as prevents its changing powers with the conductor.

Tie the two ends of a piece of silk near the two ends of a knitting-needle, so that, when you loop the middle of the silk, the two sides and needle may make an equilateral triangle; stick a bottle cork on each end of the needle, and suspend it by silk from the loop, that it may hang two inches above the conductor. When you electrify it with the vitreous power, of an excited glass, the corks are repelled from the denser to the rarer parts of the vitreous atmosphere of the conductor, and stand at right angles with the conductor; but then touch it with your finger, or wire, that it may be electrified with the resinous power; or, for more satisfaction, electrify the needle with excited sealing-wax,

wax, and the corks are immediately attracted from the rarer to the denser part of the vitreous atmosphere of the conductor, and the needle stands parallel to the conductor.

After all that has been said of the wonderful power of points, I shall now shew that they do not act one inch beyond the electric atmosphere; nor do they act upon that any farther than they are immersed in it; and indeed so far it acts upon them; by drawing the resinous power in through them, and parting with so much of the vitreous power as comes in contact with them, but no more. Suspend a piece of down by silk, so that it may hang against the side of the conductor; when you turn the wheel it will be electrified with the conductor, and fly off to the extreme part of the atmosphere of the conductor: stop the wheel, and bring your point to the outside of the down, instead of the down's being driven in toward the conductor, it will be attracted to the point, till it has exchanged powers with the point, and then it will be attracted to the conductor, and be electrified again, and repelled, and when it comes within a certain distance of the point, it will be attracted to it, as to any other non-electric; and so on, while the conductor remains electrified. When the down stands thus electrified on the atmosphere of the conductor, immerse your point a little way into the atmosphere, and you'll see the down approach the conductor, in proportion to the immersion of the point, and

and so on, as often as you move the point forward to the conductor, but no farther, hold the point there as long as you please, which plainly shews that the point acts no farther than contact in drawing off the electricity. Indeed, while the wheel is turning, and the point immersed in the atmosphere, there is a strong stream of the resinous power flowing in from the point to the conductor, and that in proportion to the vitreous power carried off by the point; (for the electric powers do change place more easily through points and slender bodies than when their surfaces are opposed to each other in any considerable quantity) and if this stream meets an electrified cork ball, or piece of down, it will change their powers, and electrify them with the resinous power, by which means they are attracted to the conductor, and will be fixed there by the continual stream of resinous power. But, draw back your hand into the rarer part of the atmosphere, to lessen the resinous stream; and you'll see the down get off from the conductor by degrees, and hang between the two powers, without being forced into the conductor, or able to fly far from it; and upon this doctrine, of one power going out and another coming in, depends the suspension of gold-leaf between an electrified plate, and a non-electric beneath. I dance half a dozen paper puppets, about two inches high, between my table and a large tin plate, wax-

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ed round the edges, and suspended from my conductor; for, as the heads are electrified with one power, and the feet with the other, they are repelled at both ends, and, therefore, never come in contact, except the plate is raised high enough to let the lower part of one touch the higher part of another; and then they attract each other, and stick together. This dance has a droll appearance, and I have scarcely met with any one who could look on without laughing: but it requires some skill and practice to shape the puppets, for a very minute alteration will make them dance, or remain fixed to the top or bottom; for, the upper part must be so much larger than the lower part, as to contain a part of the power going in, as much greater than that going out, as will be equal to the gravity of the paper; but, from a little practice, I can make one of them dance for some minutes, without touching top or bottom.

But, to return to my points. It is evident, from all the experiments that can be made, that points have not any power ~~on~~ the electric atmosphere at the distance of one inch; how is it then possible to suppose they can draw electricity from the clouds at a thousand yards distance? for so far, and often much farther, is the seat of thunder generally placed from us. I know that very magnificent ideas of the extent of the electric atmosphere of the clouds have been formed, and delivered to the public, as if they may extend

extend to these points on or near the surface of the earth; but upon what foundation in reason these suppositions are supported, I must own I cannot see. For it is plain that the electric atmosphere of a thunder cloud can only be a continuation of the electric atmospheres of all the drops; for we cannot suppose it one atmosphere to one body 'till all the drops are joined in one body, and then indeed it may be very extensive. For if we consider how this electric atmosphere is formed, the extent may in some measure be guessed at, at least I think the method of computing will be reasonable. If every particle of vapour carries up an electric atmosphere proportioned to its diameter, after a coalition to form drops, the diameter of the electric atmosphere of the drop must be in proportion to the diameter of the drop, as the diameter of the atmosphere of the particle was to the diameter of the particle, which can never amount to this monstrous extent. But I am running from my purpose, which was to explain the phenomena of electrical experiments, among which I think the charging and discharging Mr. *Muschenbroeck's* bottle has raised the attention of the inquisitive; I shall therefore explain the mystery of that phenomenon. In doing of which I think I shall shew that the electric powers, when in equilibrio, do really condense each other, and that the expansion of one power always follows upon the withdrawing the attraction of the

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other power ; which expansion is in proportion to the increase of one power and the diminution of the other power.

Place your bottle upon an electric stand with a communication from the conductor, then turn the wheel and you will find the bottle electrified on both sides with the vitreous power ; which may be proved by touching them with the down suspended by silk, for when it is electrified by touching the outside, it will also be repelled by the hook, or ball, which communicates with the inside of the bottle ; when you apply a non-electric to the conductor, all signs of electricity are drawn from the conductor, but the bottle remains in some degree charged ; which degree will increase with, and be in proportion to the time of turning the wheel. Try this experiment with the communication to the coating of the bottle ; you will find the top of the bottle, and the outside both electrified with the vitreous power, as before ; but when you discharge the conductor by touching it, the bottle is also entirely discharged. The reason of these different effects I shall explain presently. The bottle I commonly use is a quart bottle filled with shot and water, with a wire thrust through the cork and a pistol ball at top ; the body is wrapped round with two or three turns of common sheet lead ; for I observe it is necessary to have the non-electric on the outside equal to that within ; and the more there is of both, the higher your

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bottle will charge. But the degree of charging depends upon the degree of separation of the powers between the cushion and globe; and therefore much depends upon having a proper cushion. I have found a pig's bladder placed on linen or paper a very little damp, but not so much as to make the bladder moist, to answer well. But a moist or wet cushion will never produce a spark from the conductor; because then the surface of the globe becomes non-electric by being wetted, in which case the powers cannot be separated. So that I fear Mr. *Franklin's* method of collecting electricity (which is the basis of his famed hypothesis) by rubbing the sea-water against the salts which are dissolved in it, will never do. I place my bottle on a round board, upon the necks of three clean bottles. From a bottle thus placed a wire communicates with the conductor, and another from the coating to the table; when the wheel turns you will not find any signs of electricity in the coating when you touch it with the bog-down; but the top is electrified with the vitreous power, and as often as you remove the wire communicating with the table, you will find the coating also electrified with the vitreous power, 'till the bottle is full charged. Then remove your wires from the conductor and table, and touch the coating with your finger; the down will hang suspended by it without any appearance of being electrified; then touch the bullet with your finger, and the down will

will be strongly repelled from the coating, and be electrified with the resinous power. Then touch the bullet with another piece of suspended down, and the down will be electrified with the vitreous power and be repelled; then touch the coating with your finger, and the down is repelled much farther by the bullet; but the down which was repelled from the coating is now attracted to it, and remains quiet there, till you touch the bullet with your finger, and then it is electrified as at first and violently repelled, and the down which was electrified by the bullet is now attracted towards it. And this change in the extent of the atmosphere of the different powers is almost instantaneously made, as often as you touch the bullet or coating. Or you may place your wire from the bullet to the conductor, and suspend a piece of down to touch the conductor; when you touch the coating the down is repelled from the conductor, and that next to the coating is attracted to it; then touch the bullet, and the down is repelled from the coating and attracted to the conductor; and so on, as often as you touch the bullet and coating. From hence, I think that two things evidently appear; first, that the bottle is electrified with the vitreous power on the inside, and with the resinous power on the outside. Secondly, that when the balance of these powers is destroyed by lessening the quantity of one, the extreme part of the other expands itself into an extensive atmosphere, and that the atmosphere

of the lessened power is condensed, as appears by the down's falling close into the conductor or coating. But here a more mysterious phenomenon appears, which has not been taken notice of, (nor indeed was it possible to take notice of it, until it was understood that there are two powers in electricity) that the extreme parts of the vitreous and resinous atmospheres meet on the outside of the bottle without attracting each other so as to mix. Which I think may be thus accounted for. The electric powers are attracted to matter (the bottle) according to the quantity of matter, therefore the most condensed and attractive parts of these powers must meet at the glass; and the attraction of each proceeds through the series of particles of the other power to the extremes, consequently the extremes cannot attract each other with a greater power than they are attracted themselves by the densest and most attractive part of the opposite and attracting power at the glass. It now remains to shew how these powers came to be situated on the inside and outside of the bottle; or why they do not mix through the glass where their strongest attraction is. We must first consider the separation of these powers between the globe and cushion, for, according to the degree of separation there, all the rest must follow. The globe parts with its resinous power to the cushion in exchange for the vitreous, the conductor in like manner to the globe, and the inside of the bottle in like manner to the conductor; and so the exchange would go on with the
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next non-electric, but that the bottle gives some obstruction to the passage of the electric powers; by which means the vitreous power which passes through the glass, or round by the outside of the bottle, to the non-electric upon the outside of the bottle, is carried off, together with the vitreous power of the coating, along the wire which communicates with the table, in exchange for an equal quantity of resinous power brought back by the wire to the coating of the bottle; 'till at length the resinous power on the outside becomes a balance in attraction to the vitreous power on the inside; by which means the resinous power is drawn from the inside of the bottle to the conductor; so that the bottle remains a partition between these powers; and they cannot change place through the fine pores of the bottle, while their surfaces are opposed to each other in such quantity. For you see when the juncture is made in open air, where their surfaces are opposed in any quantity, it is not done without violence; for when you compleat the circle with flat surfaces a little convex in the middle, the consequence is a loud crack and flash of fire, by bursting through each other. For where the different powers attract each other in any quantity, they are much condensed, which I could prove by experiment. This would also prove other matters which I would willingly speak to, but I have not room for every thing. The violent convulsion felt through the body, by compleating the circle with the hands, is only occasioned by the different powers passing in

opposition through the same nerves. For let one man touch the coating, and another touch the top of the bottle, the bottle will be discharged without shocking either of them; tho' it is evident that as much fire passed through each, as if each had singly discharged the bottle. But in this case the fire is diffused through all parts of the body, and the fire brought in is drawn from all parts of the body, therefore the nerves cannot be so much shocked as in the first case, where all the fire passes, in opposition, through the same nerves. The reason why the bottle set on a glass stand, in the first mentioned experiment, without any communication with the table, remains charged after turning the wheel for some time, and then discharging the conductor; is because the vitreous power at the outside drew up some of the resinous power from the table through the sides of the glass stand (for I shewed before that there are not any bodies perfectly electric, and that the powers may be changed in glass) or perhaps from the damp of the air, &c. but this adventitious resinous power has no way of retreating upon discharging the conductor of its electricity; and therefore by its attraction of the vitreous power in the bottle retains an equal quantity of it there, by which means the bottle is so far charged, and remains so 'till discharged in the common way. But in the second experiment where the wire from the conductor is fixed to the coating, whatever

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resinous power is drawn from the table to the coating passes on in exchange to the conductor, globe and cushion, and there being no way for the resinous power to come at the inside of the bottle; upon discharging the conductor, the bottle is found entirely discharged, with its powers in equilibrio in their natural state. But the most mysterious phaenomenon of this bottle still remains, without any notice being ever taken of it, that I can find; which is, that after the bottle has been highly charged, and then all communication removed, and the conductor discharged that no electric atmosphere may be near the bottle; after you discharge the bottle so far that no signs of electricity appear upon touching the coating with the finger of one hand, and the bullet with the finger of the other hand; yet upon letting the bottle remain two or three minutes upon the stand, you will find it in some measure charged again, so as to yield a spark and give a shock to be felt in the wrist; and after that discharge let it rest three or four minutes more, you will find another charge that may be felt as far as the knuckle or back of the hand, upon discharging the bottle with a finger of each hand. And thus by letting it rest for longer intervals you will find it revive for five or six times, the sparks growing smaller in each succession. If the bottle be discharged with a wire fixed to a glass or wax handle, the revived charge (as I may call it) will be the stronger. This effect

I must own at first surprized me, and seemed very difficult to explain; but I now think it may be explained by the laws which I have given, and accounted for in this manner. I think that it may be easily conceived from the manner in which a bottle is charged, that there is a greater quantity of the electric powers affixed to a bottle highly charged, than would be affixed to that bottle by its mere attraction of those powers by its quantity of matter. When this bottle is discharged by a free communication from the inside to the outside, these powers are equally mixed among each other, that is equal quantities of resinous and vitreous within; and equal quantities of resinous and vitreous without; and the quantities of both equal within, and without. They then attract each other equally, and therefore a communication from one side to the other, produces no change in these powers, and therefore no sensible action. But these powers being more than the bottle can hold by attraction, one of them begins to move off from the outside to the adjacent matter, and upon that subtraction, a different power also gets off from the inside, in proportion to that which has quitted the outside; now tho' the quantities within and without are equal, yet they are not equal as to themselves, that is, there is more of the vitreous than of the resinous within, and more of the resinous than of the vitreous without; so that when a communication is formed, the bottle appears to be charged according to the disparity of the powers

ers within and without. Thus this exchange goes on to the fifth or sixth succession, or farther according to the intervals of each trial; but if time sufficient be allowed at first, the equilibrium will be restored in one discharge.

There are many complicated cases of this bottle which I think have not been taken notice of, two or three of which I will mention. First, when the bottle is fully charged by the wire, and the wire removed from the table; you may shift the communication with the conductor, from the top of the bottle to the coating, and by continuing to turn the wheel, the resinous power, with which the coating is electrified, becomes covered with the vitreous power, and you may take as many snaps from it as you please; or electrify as many bodies by it with the vitreous power as you think proper, yet it makes no alteration in the charge in the bottle: for when you stop the wheel and remove the communication with the conductor, and touch a finger to the outside of the bottle, all signs of the vitreous power disappear; and the resinous power appears upon the coating, when you touch the bullet with a finger; and when the circle is compleated, the bottle is discharged with as loud a report as it would have done before you applied the communicating wire to the coating, for the vitreous power within the bottle being undisturbed, kept an equal quantity of the resinous power firmly fixed to the outside of the bottle. But when the bottle is fully charged, and you shift the
wire

wire from the conductor, to the coating, from the top of the bottle, after the wheel has turned some little time; if you attempt to take a spark from the bullet, you will receive it with violence at double the distance you can draw a spark from the conductor; the reason is, that the vitreous power, covering the resinous power on the outside of the bottle, takes off the attraction of the resinous power from the vitreous power at the inside, and therefore leaves that power with greater freedom to fly off; but as you take the snaps the sparks lessen at every snap; because the vitreous power in the bottle is lessened, and the resinous power, within, encreased by the quantity taken in, in the exchange on every snap. And thus in a few snaps the bottle is discharged; but if you go on to take more snaps, the bottle is recharged with the resinous power in the inside, instead of the vitreous with which it was charged before. Here is another case. Suppose an hundred turns of the wheel will charge your bottle; turn fifty of them, and then remove the communication between the coating and table; as you turn on, (whether you continue the communication from the conductor to the top of the bottle, or shift it to the coating) you will find the bottle electrified with the vitreous power within and without; then with wax or glass, remove the wire which communicates with the conductor; and with a wire, fixed to a wax or glass handle, discharge your bottle; there will

will be as loud a report as you were to expect from fifty turns ; but the bottle will remain electrified with the vitreous power, both within and without ; and this electricity will be discharged both within and without, by touching either the bullet or coating with your finger. Here I could add much in regard to the passage of the electric powers through glass ; and various other matters. But if I were to explain all that occurs I must write a long letter indeed. But as it is reasonable to suppose that you will now be pleased to come to the end of this, I will conclude with requesting that any of the ingenious gentlemen of the society, who have been conversant with electricity or magnetism, will tell me honestly where they think I am wrong, or where I have been deficient in explaining my meaning, or to communicate such effects as they think cannot be deduced from the laws of electricity which I have laid down ; or that they would be so good as to communicate any thoughts of their own towards establishing some fixed doctrine of electricity, that we may not be led any longer by vague conjectures, but be enabled from some fixed laws to deduce the use of electricity in the operations of nature. I have observed Mr. *Watson's* and Mr. *Canton's* names in the *Philos. Transactions*, I should be obliged to these gentlemen in particular ; for I live in a neighbourhood where there is not a single man, who has any taste for researches of this kind ; and as custom

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tom in a great measure governs the world, when I am quite well, and at liberty, I am employed in the amusements of the place; but I am subject to long confinements by the gout, and in them I attempt something of this kind, which if it appears to the Society, or to you, to be trifling, or incorrect, you see the reason why.

It may be now objected, that the laws I have laid down are not coincident with the laws of gravity or attraction. I readily grant it; for the one is a principle of rest, and the other a principle of motion; therefore their laws cannot be the same. And, though electricity itself in some measure seems obedient to the general attraction, as it is found adhesive to all bodies in proportion to their quantity of matter; yet, in numberless cases, it does counteract and overcome the power of gravity.

Though I am conscious that this letter is too long, yet I also know that I have omitted much which is to the present purpose; and I must make it longer, by making an apology, in saying how difficult it is to produce a new doctrine, and to establish it by experiments, in the compass of one letter, after all that has been said by so many considerable men on this subject. But how far I have done this, is submitted to the judgment of the Royal Society. I am,

your most humble, and
most obedient servant,

HEN. EELES.

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I am something like a fellow at the gallows, who often takes leave, but is very unwilling to dispatch the audience; so am I plaguing you with postscripts. There is one observation which obtrudes itself, which I must beg leave to mention. I have said that the electric powers are adhesive to all matter; it may be asked, why not then to the particles of air? I answer, from many experiments and observations which I have made, I think it highly probable that they are; and yet that all the electrical experiments which we make, may be carried on in the air, in the same manner we find they are; and, from many uses of the air in nature, I think it highly necessary that the electric powers should be joined with it; and many effects of the air, shew that they really are. But here I foresee a ready objection to what I have said of the ascent of vapour and exhalation, viz. That if the electric powers are added to the particles of air, in proportion to their specific gravity, how can the affixing it to particles of vapour in the same proportion render them specifically lighter than air? I answer, that the extent of the atmosphere affixed to each particle must be in proportion to the mixture of the electric powers adhering to that particle, and consequently the particles of vapour may have much more extended atmospheres than the particles of air; they being detached from gross bodies, whose surfaces may be electrified;

ed, as we call it ; and therefore the particles detached from them may bring off the electric powers affixed to each particle in very different quantities. And thus, my system of the ascent of vapour and exhalation, thunder, rain, &c. may be better explained than before. But this would require a paper by itself ; therefore, for the present, I acquiesce.

I believe, I should have mentioned the cylinder I use, because it is but small in proportion to many others, that the experiments may be better understood ; and, also, to shew another circumstance in the easy fitting up of globes, or cylinders. Mine is a clear glass bottle, four inches and an half diameter, and seven inches long in the side, with an iron axis passed through the middle, which does not prevent the working of the bottle ; for the ends of the bottle, the iron axis, and the pikes of the lath, upon which it turns, are all electrified with the resinous power, while the wheel is turning ; which shews that the vitreous power does not fly off that way : and a communication from the pike to the coating of the bottle, will make the bottle charge sooner and higher, than a communication with any other non-electric ; because the resinous power is there prepared to fly off to the bottle.

Here, I could tell how heat excites, and friction separates the electric powers ; but this would require a recital of experiments too prolix to insert.

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I must beg the favour that you will let me know, if this letter gets to your hands, or whether you received mine of the 9th of *August* last.—Wherever I mention down, I mean bog down, and not the down of feathers, which will not answer in these experiments, being itself an electric *per-se*.

* * Sent this letter, dated 14th *February*, 1758, to the Reverend *Thomas Birch*, secretary to the Royal Society, in Crane-court, Fleet-street, London.

L E T T E R VII.

S I R, *Lismore*, 3d *January*, 1760.

I SEND you by this post a letter for the royal society in two packets, and the inclosed. My design in this letter is to give a farther explanation of the cause of thunder, and of the descent of vapour and exhalation, &c. But as my reasoning in this letter is founded on a doctrine of electricity, which I communicated to you in a letter of the 14th of *February*, 1758, and that I find that that letter is not inserted in the *Philoso. Transactions*, I am doubtful that this letter may be thrown by with the same inattention. All I can say is that I am fully ascertained, by numberless experiments, of the truth of that doctrine; and that I am persuaded, that, if any of the gentlemen of the Royal Society had examined

amined it carefully, they also would have found it to be true. I may add farther, tho' it may appear presumptuous in me, that it is the only doctrine of electricity extant, (which I know of) which can fully explain the electrical phænomena. For what Mr. *Franklin* has said, (tho' an ingenious, and I believe an ingenious gentleman) that electricity resides in glass in such a manner that when one side of the glass receives an additional quantity of electricity, (which he always supposes that it does, when it is electrified; tho' this is also a mistake) the other side always parts with an equal quantity of its natural share of electricity; so that, according to his explanation of charging the *Leyden* bottle, one side must be electrified negatively, or minus; that is, want as much of its natural share of electricity, as the other has over and above its natural share. I say, that this is all a mistake; for glass may be electrified at both sides, at the same time, either by the vitreous or resinous power: and, therefore, in the first case, both sides must abound with electricity, and, in the next, both sides must want of their natural share. Which quite oversets his doctrine of electricity in glass. A thin plate of wax may also be electrified at both sides at the same time, by either of the powers. And, what is more, I can electrify the glass in either manner, by electrified wax; or, I can electrify the wax in either manner, by electrified glass; or, I can leave the different sides of the wax, or glass,

glass, electrified by the different powers of electricity at the same time. Which matters can never be explained by positive and negative electricity, or a plus and minus of the same power. Nor can any of the phenomena of electricity be explained by the action of one single power; since it is evident that they depend upon the actions of two different powers, and their manner of acting on each other.

Mr. *William Watson* in his experiments in *vacuo*, in the 47th vol. of *Philos. Transf.* thinks that he sees the electricity passing in through his *vacuum* to the machine; but in this he sees but in part; for there is as much of one power going out as there is of the other coming in; for these powers are never rendered visible but in passing from one body to another in opposition to each other. And this he may try by placing his *vacuum* upon wax, and applying the wire of a *Leydenphial*, while the coating is held in his hand, to the farther end of his *vacuum*, and he shall find the phial charged with the power which comes out from the machine, which will be contrary to that which goes in. And I am confident that if I were with the gentlemen of the Society, I could easily convince them of the truth of my doctrine. All I require is, a careful examination of it; and that, if in any part of it I appear unintelligible, I may have an opportunity of explaining the matter more fully.

As the electric powers are a great principle of motion in the world, it is certainly worth

the while to enquire into the truth of a doctrine which so easily explains all the known phænomena of electricity in its natural and artificial operations ; which, without it, are inexplicable.

As I do not know that any writer upon electricity has considered it as consisting of two different distinct elastic powers, I am the less surpris'd that my doctrine is not readily accepted ; but surely, if it be true, it is not less a truth for my having singly hit upon it. A truth which will greatly conduce to speculative knowledge, and that it may be applicable to practical knowledge, I have often experienced, particularly in the curing of the human body.

I shall only trouble you with one experiment more, which, if any of the writers on electricity can explain by any thing which I have seen published about positive and negative electricity, or a plus and minus of the same power, or by glass throwing out and wax drinking in the electric power, or by any supposed action of one single power, I shall readily grant that I have troubled the Royal Society, with my doctrine of two different distinct elastic powers, to very little purpose.

I have two cylinders, one of glass and one of sealing-wax ; each about five inches diameter. Which cylinders I can make exchange offices as often as I please ; that is, I can make the glass throw out the resinous power, and electrify the conductor and all other matters

ters communicating with it in the same manner that wax generally does; and I can make the cylinder of sealing-wax, throw out the vitreous power, and electrify the conductor and all other matters communicating with it, in the same manner that glass generally does. Which shews, that both the different powers exist together both in the wax and in the glass, since I can alternately throw out which power I please, either from the wax or from the glass.

I can, with a rubbed piece of sealing-wax, electrify a piece of bog down suspended in silk with the vitreous power, and then retract that and electrify it with the resinous power, and thus change the powers in the down several times without any fresh rubbing of the wax, or touching it to any other thing, which demonstrates that both the powers exist together withoutside the surface of the wax. Nor shall a spectator perceive any difference in my manner of touching the down with the wax. These facts I have shewn to hundreds of people; who can attest the truth of what I write; and many more experiments, which demonstrate that two different distinct elastic powers of electricity do exist together in all bodies.

And upon these powers being equally attracted by all matter, and their equal and strong attraction and condensation of each other, and their great elastic power, depend all the phenomena of electricity; and this

will be found true while things subsist in their present order. For the Creator established the principles of motion and rest invariable ; for should they alter or cease, all must fall into wild disorder and confusion.

And now, sir, I must beg leave to address you in your private character, and to request a favour which I hope you will not refuse, which is only to let me know whether my letters of the 9th of *August*, 1757, 14th of *February*, 1758, and 26th of *February*, 1760, got to your hands. For if you received them, and they were not thought worth the attention of the Royal Society, it may be impertinent to trouble them, you, or myself with any more essays on this subject, tho' it might well employ many hands for much more time to come.

I have been applied to by some friends to write something systematical on electricity, and particularly by some physicians, to explain my method of applying the electrical powers to medicinal use ; but if my papers are accepted by the society, I should rather chuse to write essays than systems ; but if not, I shall endeavour to leave my discoveries and opinions of these matters to the world in another course.

I am, S I R,

your most humble and

most obedient servant,

HEN. EELES.

** Sent

* * Sent the foregoing letter, dated 20th of *March*, 1761, to the Reverend *Thomas Birch*, Secretary to the Royal Society.

L E T T E R VIII.

Lismore, 14th *October*, 1760.

GENTLEMEN,

HAVING lately received the volume of *Philos. Transactions* for the year 1757, I was surpris'd at meeting with two letters signed *Erasmus Darwin*, Physician at *Litchfield*, *Staffordshire*, beginning at page 240, which that gentleman is ~~placed~~ *pleased* to call a confutation of what I wrote to you on the cause of the ascent of vapour and exhalation. Indeed with the liberties which he has taken, he may confute or rather confound any author that ever wrote. But I believe this gentleman is but a young philosopher, for he appears more studious to make a rhetorical flourish, than to explain the matter which he writes about. I shall beg leave to transcribe his first paragraph, because I think it sounds well, tho' not much to the purpose.

' There is ever such a charm attendant upon novelty, that be it in philosophy, medicine, or religion, the gazing world are too often led to adore, what they ought only to admire: whilst this vehemence of enthusiasm has generally soon rendered that object contemptible, that would otherwise have

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‘ long laid claim to a more sober esteem. This
 ‘ was once the fate of chymistry; the vain
 ‘ and pompous boasts of her adepts brought
 ‘ the whole art into disrespect; and I should
 ‘ be sorry, if her sister electricity should share
 ‘ the same misfortune. It is hence the inge-
 ‘ nious Mr. *Eeles* will excuse me, for endea-
 ‘ vouring to lay before you my opinion on the
 ‘ ascent of vapours, tho’ it by no means coin-
 ‘ cides with that he is so strenuous to establish,
 ‘ and plucks a plume from his idol goddess,
 ‘ electricity.’

Here I think this gentleman might have distinguished better between art and nature; it is true that an art may suffer by the imprudence of its professors; but dame nature will go on with her work, whether we talk wisely or otherwise about her; and therefore he may lay aside his concern. He has indeed told me two things which I did not know before, that electricity was a feathered goddess and an idol of mine; since he will have it so, he may have a plume for his pains when he can pluck it.

He begins by telling you that I have said, that every particle of vapour is endued with a portion of electric fire; and that there is no other sufficient cause assigned for their ascending. It is true that I then thought so, and still do think so, and had I known of any sufficient cause assigned, I must have been a most ignorant, impudent blockhead, to think of palming any other on the Royal Society. He then goes on thus.

‘ My

My design is therefore first to attempt to shew, that another theory, founded on principles better known, will sufficiently explain the ascent of vapours: and then, that some kinds of vapours are not endued with a more or less than their natural share of electric æther.

The latter part of this design I shall readily grant him is true, but for some reasons which this gentleman seems a stranger to; that is, that all bodies have a quantity of electricity nearly in proportion to their specific gravities; and that bodies do not become electrified by having a greater or less portion of electricity in general; but by an encrease of the one, and a proportional lessening of the other power of electricity; for when the attraction of the lessened power is diminished in proportion to the quantity of the power; the encreased power expands itself into an extensive atmosphere; which is generally called electricity. So that those who talk of an accumulation of electricity, do not understand the action of these powers. This matter I have explained in a letter to Doctor *Birch*, of the 14th of *February*, 1758, and there given the general laws by which these powers act on each other, and on the other parts of matter.

As to this gentleman's new theory founded on well known principles; I do think that it was well known that fire made the pot boil, and that the steams arose; but when he comes

to shew us how, it is by the immeasurable expansion of water in steam which renders it specifically lighter than air; but I believe that he is the first philosopher who ever did, and perhaps the last that ever will take steam, vapour, or cloud to be one continued expanded mass of water; for he himself in the next page speaks of the spherules and globules of vapour. In my letter concerning the cause of the ascent of vapour, &c. I have demonstrated that the specific gravity or expansion of a body is not altered by dividing that body into minute parts; therefore, until he has shewn that every particle of vapour is a thousand times more expanded than it was while a constituent part of the watry mass; it is impossible for him to shew that it is sufficiently specifically lighter than air, so as to ascend in it by the laws of specific gravity. And here I may rest this matter; but as there is something extraordinary in this gentleman's manner of reasoning, I beg leave to make a few observations.

First, he does not make any distinction between the expansion of the parts of bodies, or their being set at a distance from each other by some repulsive power; so that the immeasurable expansion and condensation of vapour, which has puzzled him, may not be in the watery particles, but in that subtile power which keeps them asunder. And that it is so, I think I can easily prove from the nature and actions of the electric powers; which powers
will

will fully answer almost all the purposes (except that of gravity) for which the great Sir *Isaac Newton* sought for such subtile powers of attraction and repulsion. And here I must beg leave to transcribe a few lines from that great author, where he supposes the necessary existence of such powers; especially of repulsion.

“ And that there is such a virtue, seems to
 “ follow from the reflections and inflections of
 “ the rays of light. For the rays are repelled
 “ by bodies in both those cases, without the
 “ immediate contact of the reflecting or in-
 “ flecting body. It seems also to follow from
 “ the emission of light; the ray, so soon as it
 “ is shaken off from a shining body by the vi-
 “ brating motion of the parts of the body, and
 “ gets beyond the reach of attraction, being
 “ driven away with exceeding great velocity.
 “ For that force which is sufficient to turn
 “ it back in reflection, may be sufficient to
 “ emit it. It seems also to follow from the
 “ production of air and vapour. The parti-
 “ cles when they are shaken off from bodies
 “ by heat or fermentation, so soon as they
 “ are beyond the reach of the attraction of
 “ the body, receding from it, and also from
 “ one another with great strength, and keep-
 “ ing at a distance, so as sometimes to take
 “ up above a million of times more space than
 “ they did before in the form of a dense body;
 “ which vast contraction and expansion seems
 “ unintelligible, by feigning the particles of
 “ air

“ air to be springy and ramous, or rolled up
 “ like hoops, or by any other means than a
 “ repulsive power. The particles of fluids
 “ which do not cohere too strongly, and are
 “ of such a smallness as renders them most
 “ susceptible of those agitations which keep
 “ liquors in a fluor, are most easily separated
 “ and rarified into vapour, and in the lan-
 “ guage of the chymists, they are volatile, ra-
 “ rifying with an easy heat, and condensing
 “ with cold.” *Newton's 31st Query.*

Now it is evident that this great man never
 imagined that this vast expansion and contrac-
 tion of vapour was in the particles of vapour,
 but in that power which set them at a dis-
 tance from each other. If the particles are
 not expanded, how do they become specifi-
 cally lighter than air? If *Mr. Darwin* takes
 in the repulsive power to make out the ex-
 pansion, he then proves my system; if not,
 he has proved nothing toward the cause of the
 ascent of vapour.

And here, gentlemen, be pleased to observe
 the great conformity that there is between
 what this great man has said about the pro-
 duction of vapour, and what I have said on
 the same subject, in my letter concerning the
 ascent of vapour; tho' he only reasoned from
 the effects to a supposed cause, and I reasoned
 from a known cause to the effects, without
 having his doctrine in view; nor do I recollect
 that I ever read his queries till long after my
 writing that letter; when they were occasi-
 onally put into my hands by an ingenious
 gentleman,

gentleman, who had read Sir *Isaac's* works many years later than I had done. As I had tried many experiments on the human body, in successfully curing paralytic, rheumatic, and other disorders, merely by the electric powers; and observing their wonderful effects on the nerves in restoring them to a due modification; so as to enable them to perform their proper functions, I was led to think that our senses may be performed by these powers; particularly, seeing and hearing. On my telling this gentleman my opinion, he desired me to take care not to give that as my own, which was said by another; and then brought Sir *Isaac's* Queries, wherein, especially at the 23d query, I found, very nearly, what I had said to this gentleman. But to return to my subject.

I need not have applied to so great an authority, as Sir *Isaac Newton's*, for the formation of vapour; since it is evident to the senses of every man, who will enquire properly, that vapour is emitted in exceeding minute, distinct particles, and that it does ascend in that form; and that above there is a collision of these particles to form larger particles, such as compose mists, and that they farther coalesce 'till they come down in mists, rain, snow, hail, and such heavy drops as form thunder-showers. When I found that all ascending vapour was really electrified, I thought that the expansion of that power was sufficient to carry up the particles; and, to bring them down again, I used

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the most probable means I could then think of. However, there was one thing which I could not possibly explain; that is, how the whole quantity of electricity, which ascended, was ever to return to the earth again. And this enquiry prevented my sending you that letter, for at least a year; 'till at last, tired with the search, and finding the rest of my system very conformable to the phænomena of nature, I let it go with that known defect. And in this puzzle I might have gone on to the end of time, if I had not discovered, upon a farther research into the nature and actions of electricity, that electricity consisted of two different, distinct, elastic powers, which always strongly attract and condense each other; and, therefore, when they are equal in quantity to each other, they condense each other into so small a compass, that they exert but little sensible action; but when the equality of quantity is altered by a diminution of the one power, and an increase of the other power, then the increased power has a power of expanding itself in proportion to the subtraction of the other attracting power, by which means it forms itself into what we call an electric atmosphere; which atmosphere will repel any other atmosphere raised from the same power of electricity, but will attract any atmosphere raised from the other power of electricity. These atmospheres are always formed with an increment of rarity as they recede from the lessened attractive power, which lessened power
always

always acts inward to the body which is electrified. These powers I have before called the vitreous and the resinous, only for distinction, as the glass generally throws out the first, and wax the second; not but that I can raise either of the powers from the glass, or from the wax; nor do I know of any different essential characteristic to distinguish them by. These powers are equally and strongly attracted by all matter; and are always found adhesive to all matter, nearly in proportion to the specific gravity of the matter, as far as I have been able to judge. Nor is it easy to subtract any part of either of these powers from any part of matter, without adding an equal portion of the other power; which addition it will retain, until it is supplied with an equal quantity of the power first subtracted. So that when any body is electrified with the vitreous power, and you take a spark from it with your finger, there is just as much of the resinous power passes from your finger into the body, as there is of the vitreous passing from the body into your finger.

Now, the general law by which these powers act on each other is this; that a vitreous atmosphere repels another vitreous atmosphere, with a force exactly equal to that with which it attracts a resinous atmosphere of equal density with the repelled vitreous atmosphere: and so, *vice versa*, of the resinous atmosphere.

And, the attracting and repelling atmosphere (of either kind) acts with a force proportional

portional to, and encreasing with, its own density: That is, the farther a body is immersed into an electric atmosphere, the greater force it is attracted with; as all electric atmospheres have an increment of rarity outward from the electrified body. But, for a farther explanation of these laws, and experiments to prove them, I must beg leave to refer to my letter to Doctor *Birch*, of the 14th of *February*, 1758.

From a great number of experiments which I have made, I find that the clouds are electrified, sometimes with the one power, and sometimes with the other power of electricity; and that the change is often very sudden; for one cloud shall pass strongly electrified with the vitreous power, and another shall approach, in three or four minutes, as strongly electrified with the resinous power.

But I never had an opportunity of observing that this change happens so often as Mr. *Canton* says it does, sometimes five or six times in half an hour. And I hope this ingenious gentleman will excuse my telling where his mistake may arise from his manner of trying this experiment. His method is with two very small cork-balls, suspended from the end of a wire by linen threads; and then, (when they are electrified) by bringing rubbed sealing-wax, or glass under them, he thinks they are electrified positively, or negatively, according to the attraction, or repulsion of the wax or glass. But, in this, he

he is much mistaken, by following the doctrine of positive and negative electricity, &c. and for want of knowing the real action of the different powers of electricity upon each other. For, let him fix his wire to a large body, suspended by silk, and let that body be electrified by wax 'till the balls repel each other; then let him bring wax, well rubbed, under the balls, and they shall be attracted to the wax, instead of being repelled; or, if the wax is very lightly rubbed, the balls shall be repelled from it. The reason of their being attracted is, that the wax being more electrified than the body, the resinous power of the wax repels the resinous power of the balls up the linen thread, and attracts the vitreous power of the body into the balls, which are then electrified by the vitreous power, and, consequently, attracted by the wax. In the second case, where the wax is but little electrified, it repels the balls, without forcing their resinous power up the threads. But, the way to try this experiment with more certainty, would be to suspend a piece of bog-down by silk, a foot long, from the end of a stick; and then, touching the down with the end of a long wire, the down will be electrified; and then you may know by which power; by the rubbed wax, or glass; as the powers cannot change place so easily through the silk, as they do through the threads. There is another great mistake to be corrected; for, he seems to think that the balls are electrified with

with the same power with which the clouds are electrified; but the balls are always electrified with the contrary power, which is drawn up from the earth through him by the opposite power of the clouds. And this may be easily tried, by placing a long iron rod, with needles at top, in a bottle to be charged by the clouds; and he will constantly find the inside or top of the bottle, so charged, electrified with the power contrary to that with which the balls, or down, is electrified. But the needles at the top of the rod will be electrified by the same power which electrifies the down and balls; because that power is there drawing off from the inside of the bottle, in exchange for the contrary power, which comes into the bottle, while the bottle is charging. I may here appear prolix; but much more is necessary to explain this matter fully; which, for the present, I shall postpone. It would be a great matter, if we could discover how these clouds come to be electrified with the different powers; which certainly must happen in their first emission from the masses of which they are composed. The pursuit of this enquiry I would earnestly recommend.

Since it is now evident that the clouds are often electrified with the different powers of electricity, and that such clouds are often in near approach to each other; and since I have discovered that electricity consists of two different distinct powers, it is easy from the laws which I have given, for the action of these powers

owers on each other, fully to explain the cause of thunder; and to shew how the particles of vapour coalesce to form still larger particles, till they come down in mists, rain, snow, hail, and heavy showers; and bring down their whole quantity of electricity with them, which before buoyed them up. But before I proceed, I beg leave to mention one experiment which I made, to shew the amazing expansion of those powers, and through what an incredible space they will diffuse themselves by their own elastic power, when the attraction of the other power is taken off, by lessening that power as far as we can by the electrical apparatus, which is far from an entire separation of these powers; and the astonishing retraction and condensation of that atmosphere, when the powers are again rendered equal in quantity to each other. I suspended a tube of sheet lead, seven feet long, and near three inches diameter, in silk lines; I then placed a piece of bog-down on the end of a wire, on a horizontal level with the tube, and pointing towards it; I charged a phial coated with lead, and twelve cubic inches of water in the phial. Upon my holding the phial in my hand, and giving a spark to the tube, I found that the atmosphere of the tube moved some of the fibres of the down, by attraction, at the distance of more than seven feet; an assistant then took off the atmosphere of the tube, by touching it with his hand. And thus I went on for more than an hundred sparks, when the down

was moved at the distance of eighteen inches. So that, measuring these atmospheres in a decreasing series from above fourteen feet to three feet diameter, I found they contained twenty millions of times the space which the water in the phial took up. Yet all this electricity (according to the doctrine of positive and negative electricity) was lodged in the pores of the water, without the least sensible encrease of the water. But here I must not let a known error go, for the greatest part of these atmospheres never was in the phial; but each spark from the bottle added something to one power in the tube, and at the same time took an equal quantity, from the other power in the tube, into the phial; and then the encreased power of the tube expanded itself into an atmosphere, together with the additional quantity which it received from the phial. For had the tube been much less, the atmospheres would not have been so extensive; however, the expansion of that which was really in the bottle is amazing. For Sir *Isaac Newton* has shewn that light approaches to a body within the $\frac{1}{89000}$ part of an inch before it is reflected; and as I have endeavoured, in my letter to you of the 26th of *February*, 1760, to shew that the electric powers are the cause of reflection and refraction, it is highly probable that the two different powers are condensed within that distance from the surface of the bottle (and I mention this experiment mostly in regard to the reflection and refracting of light.) Now if
a small

a small portion of one of these powers can diffuse itself through a space some millions of times larger than what the whole bottle takes up, what amazing difference must there be between its greatest expansion and condensation; and yet the most extended electric atmosphere is retracted and condensed in an instant. The reason why the water in the bottle is never increased or decreased in charging or discharging the bottle, is because there is always the same quantity of electricity in the water; for as one power goes out, an equal quantity of the other comes in.

It was known to Mr. *Franklin* and others, that there was no more electricity in the bottle when charged, than there was when discharged; but till I had discovered the action of the different powers of electricity, it was impossible to explain this matter by one single power of electricity, or to explain any one phenomenon arising from electrical experiments. I have shewn, in my letter of the 14th of *February*, 1758, that there is something more of the electric powers in a bottle highly charged, than when discharged; which appears from the bottle's recharging of itself, after it has been seemingly entirely discharged.

I come now to shew how that electricity which buoyed up the vapours may be again condensed, so as to let the vapours fall in mists, rain, &c. and to return with them to the earth.

M 2

Suspend,

Suspend, by silk, two large metal tubes, or bodies of any kind, which will conduct electricity (or two pieces of bog-down, in which the attraction, expansion, and condensation of the electric powers will be more plainly seen) and electrify them ever so highly, but equally, with the different powers of electricity; that is, one with a glass globe, and the other with a globe of sealing-wax; when these bodies approach they will flash into each other, and upon contact their extensive atmospheres will be condensed into an insensible one; the different powers of electricity being then rendered equal in quantity in each body, and, consequently, condensing each other into almost an insensible compass. But if one body was more electrified than the other, then, after contact, they will both remain electrified with the excess of that power which was most in the body that was most electrified. When two clouds, which are highly electrified with the different powers, approach each other, the same thing will happen; for as soon as the extreme parts of their atmospheres begin to meet, they will attract each other with an encreasing force as they approach, till they flash in exchanging their different powers. But as clouds are formed of distinct particles, and every particle has its share of both the electric powers, and according to the equality or inequality of quantity of each power in each particle, the particle is more or less electrified; various and numerous must the ways be, in which clouds approach

approach each other, and in which they exchange their different powers.

The case above-mentioned of thunder clouds, happens when the electrified particles of the cloud are brought to approach each other so near, that their atmospheres are pressed off together to a great distance from the cloud, and act nearly in the same manner as if the cloud was one continued body; but after the flash those particles, which have fully changed powers, so as to have the electric powers equal in each particle, must have their atmospheres condensed, and consequently the electric powers are of no farther use to buoy them up, and therefore they come down as we see them do. That these atmospheres are extended to a great distance from the cloud, appears from all the foreign and domestic experiments, but more particularly from those which I have made; for in them it is plain, that an atmosphere is drawn up from the earth, of the power which is contrary to that of the cloud; which would not be if the atmosphere of the cloud did not reach the earth. When one of these highly electrified clouds approaches so near the earth as to exchange powers with the earth, then is the damage done to those things through which the exchange is made; which must be those bodies that rise nearest the cloud. And here I may quote some of the foreign experiments and observations (which the experiments could not explain) which will shew that the atmospheres of the clouds are condensed

denfied at the time of their joining with the flafh; and that the atmosphere, drawn up from the earth, is alfo then retracted again to the earth.

In the 47th vol. of *Philof. Tranf.* page 550 and 551, Mr. *Ludolf's* 4th obfervation: At every clap of thunder the electricity feemed extinct, and returned not till after thirty feconds, or thereabout, and fometimes longer. Sixth Obfervation: They put a thread upon the wire, the two ends of which hung down; which fhewed electricity, by mutually repelling each other; for at every flafh of lightning, they approached each other fuddenly, as if they had been pushed one againft the other by force. Same vol. page 557, Abbe *Nollet's* letter: And it even feemed, that the clap of thunder put a flop, for a very fhort time, to the force of the electricity. Page 559, Mr. *Mylius of Berlin*: It was alfo continually obferved, the effects were greateft when the lightning was neareft; and that, for fome moments after the lightning, the effect ceafed, but returned and encreafed by degrees.

All thefe obfervations, and many more, I have made; but to give them more weight, I have troubled you with thefe quotations. But this matter is eafily demonftrated by the electrical apparatus. Let two cork balls be fufpended by linen threads from the end of a wire, and brought within the atmosphere of an electrified conductor, they will be electrified with the power, which is contrary to that which electrifies the conductor, and repel
each

each other, but will be attracted towards the conductor; but when a flash is taken from the conductor, the balls instantly fall together; that electricity, which is drawn into them from your body, by the atmosphere of the conductor, being again retracted into your body.

It often happens that clouds are highly electrified and attract each other, and that their particles attract each other into contact, and exchange powers, without that violent flash which constitutes thunder; and that the atmospheres of the particles are by that means condensed, and lose their repulsive power, and run into farther contact with each other by the general attraction of gravity, and by accidentally impinging upon each other in their fall, and thus come down in heavy showers. Now when two clouds thus attract each other into one, the particles must attract each other into contact, and exchange powers most in the middle of that cloud, and have their atmospheres condensed, and consequently come down in the heaviest drops; and, accordingly, the heaviest part of the shower is always from the middle of the cloud. To prove this doctrine, I could produce a great number of experiments, which I have made on the clouds; but shall only mention the last, which was on the 29th of *October*, 1760, when the clouds were very distinct, and the showers heavy. In three different clouds, I found the beginning of each cloud electrified with the vitreous power; in the middle of

each shower no sign of electricity; and, in the end of each cloud, I found the resinous power of electricity. Wind, N. W. The reason why there was not any appearance of electricity in the middle of the showers, was because the electric powers were equal to each other in every drop, and therefore their atmospheres were condensed into insensible ones.

It often happens that rain, hail, and snow do exhibit signs of being electrified; because the clouds are seldom so equally electrified with the different powers of electricity, as, upon meeting, to render the powers of electricity equal in each descending drop. And this is most evident in large flakes of snow; for when they come very near a non-electric body, they are drawn to it, and cling into it, in the same manner that an electrified feather does.

Now, gentlemen, I believe I need not trouble you with a prolix detail, to shew how clouds differently electrified with the different powers may approach each other; or how the particular particles may attract and run into each other, and have their atmospheres condensed according to the different degrees in which they were electrified by the different powers; since the general laws, which I have given for the action of the electric powers, will explain all the phenomena arising from electrical experiments, which I know of, at least I never found them deficient in any experiment which I could think of making; whether

whether I reasoned from the laws to the effects, or from the effects to the laws. And, therefore, I think I may claim the honour of having discovered this new doctrine of electricity, without the knowledge of which, I think it is impossible fully to explain any one phenomenon arising from electrical experiments. But if any ingenuous gentleman can shew me where those laws or properties of electricity are wrong, I shall be really obliged to him; for I am much more desirous to discover truth, than to appear as an author.

And now, gentlemen, I hope that you will excuse my pointing out some of the absurdities of Mr. *Darwin's* theory, which contradict the reason and experience of all mankind. He first supposes that water is so far expanded by heat as to rise in the atmosphere by the laws of specific gravity; and then that it is better supported in a rarer and colder part of the atmosphere above, than in a warmer and denser part of the atmosphere below; he says, because the pressure of the atmosphere is less above, and for that reason the water can better expand itself. If vapour arises by the laws of specific gravity, it must ascend quickest where the pressure of the atmosphere is greatest, the expansion of the vapour remaining the same. And it is impossible to suppose that the expansion of vapour is increased by heat above, where vapours are ever found colder than the circumambient air, and the circumambient air there, always colder than that below. This is a fact well known

known to every man, that had his feeling, who ever passed over high mountains. He says that he is persuaded, that was it not for the pressure of the superincumbent atmosphere, greatly less than that of boiling water would instantly disperse the whole so heated into vapour. But would this gentleman have us suppose that this vapour is to arise by the laws of specific gravity, where there is not any medium specifically heavier for it to rise in? When the impulsive or repulsive force ceases, which dispersed it, must not this vapour return instantly to the earth by the general laws of gravity? Does this any way point out the cause of the ascent of vapour, or shew us how the clouds float by the laws of specific gravity, which they evidently do? Indeed he has found out one extraordinary way of supporting them, which is by the motion they acquire from the winds. This might do, if the wind blew from the Nadir to the Zenith; but as it generally blows on a horizontal level, I think it may as well blow them down as up. He then quotes the opinion of Sir *Isaac Newton*, to shew that bodies are only heated by reflection and refraction of the sun beams. How he came to imagine that this was Sir *Isaac's* opinion, I can't conjecture; for Sir *Isaac's* eighth proposition, of his second book of *Opticks*, stands thus: "The cause of reflection is not the impinging of light on the solid or impervious parts of bodies, as is commonly believed;" and this Sir *Isaac* has fully proved, both as to reflection

tion and refraction; and all experience shews that black opaque bodies, which produce but little reflection or refraction, are exceedingly more heated by the sun, than diaphanous bodies, where the reflection and refraction are exceedingly great. Place a convexo convex-glass opposite the sun, and at the same time a black body of the same magnitude, and you will find the one grow very hot, while the other retains but very little heat.

This gentleman's chief principle is, that the sun heats vapour, but does not heat the air: But, lest you may think I mistake him, please to hear his own words.

"The ingenious Mr. *Eeles*, I dare say, has already foreseen the use I am going to make of this principle; viz. That the little spherules of vapour will thus, by refracting the solar rays, acquire a constant heat, tho' the surrounding atmosphere remain cold."

I think that this gentleman should have produced some better proof of this matter, before he endeavoured to make us discredit our senses. Does not every man know the air is warmer at one time than at another, and that it is sometimes very warm? If the sun does not warm the air, I desire to know what does warm it? As to his assertion, that clouds are warmer than the circumambient air, I declare that it is entirely false; and I appeal to the experience of every man, who ever passed through them on the sides or tops of high mountains, whether he has not constantly found them to be damp, chilling fogs, much colder

colder than the circumambient air ; and sometimes the particles so large as to wet a man considerably ; tho' in 'passing from one mountain to another they do not let any of their wet fall into the valley between. All this I have experienced a thousand times ; therefore Mr. *Darwin* must excuse me for chusing to be guided by the evidence of my own senses, rather than by his assertion.

But if I must produce an authority for what is well known to so many thousands, please see Dr. *Peysoneld's* account of the *Brimstone-bill* ; 49th vol. Phil. Transf. page 571 : “ Besides the fine prospect you enjoy at the top of this mountain, you have the pleasure, as father *Du Tertre* observes, of seeing the clouds gather below, and hearing the thunder rumble under your feet. We actually saw the clouds rise from the sea, and spread over the land on the side of the wind, and sometimes passing where we stood, and sometimes lower. These clouds are no other than damp fogs. The *Brimstone-bill* is seldom clear of these damp fogs.” And in the preceding paragraph he tells you how the naked negroes often have their blood chilled by these damp fogs, so as to perish by it.

Mr. *Darwin* says, from considering this power of expansion, which the constituent parts of some bodies acquire by heat ; and withal, that some bodies have a greater affinity to heat ; that is, acquire it sooner, and retain it longer, than others ; that many things, before utterly inexplicable, became easily understood

derstood by him ; particularly how that ponderous body, mercury, may be raised into vapour. But, unluckily, he has not told us how these matters became easily understood by him, or how they may be easily understood by us. I must confess, that if he could explain how the particles of mercury may float upon the air, that then he may easily explain how the particles of water may float there ; but till he has done one or the other, I shall leave his theory to be farther considered by those who think it worth the while.

The design of this gentleman's second letter is, according to his own words, entirely to destroy the probability of my notions, and to give an additional support to his own theory. And then he says, " For this purpose, " our first endeavour will be to shew the uncertainty of some of the most material principles that support his arguments ; and afterwards the fallacy of the experiments he has given us.

" First, then, in page 130, Mr. *Eeles* has asserted, that the greatest possible rarefaction of water is when it boils."

This he has taken from a cursory observation of mine, which stands in these words, and which I think may still keep their place. " And here I must observe, that it has not yet been explained how water can be dilated, so as to occupy eight hundred, or a thousand times the space, which it naturally does. For I think, the greatest expansion it suffers by boiling (the greatest heat we
" can

“ can give it) is not more than a nineteenth
“ or twentieth part of what it was before.”

This gentleman accuses me of a confusion in terms, while himself has bred the confusion ; for he has changed my word, dilated, into the greatest possible rarefaction : By dilatation, I meant that expansion which bodies suffer, without any seeming discontinuity of their parts : Rarefaction, I know, is often applied to the dividing a body into innumerable parts. He then goes on thus.

“ In page 133, the sphere of electrical activity is said to be encreased by heat. If by
“ electrical activity is here meant an encrease
“ of its repulsive power (the thing which
“ seems to be wanted in Mr. *Eeles*’s hypothesis) I know no experiment to shew it.”

I can easily grant that this gentleman may be ignorant of experiments to prove this and many other matters that are necessary to a moderate understanding of electricity ; but if he wanted eyes, would he, for that reason, persuade us that there was not any light ? If it be required, I can produce experiments which fully prove that the electrical powers are expanded by heat : A clear knowledge of which is very necessary to a right understanding of the action of these powers in many of the operations of nature, and many of those of art, where natural powers are made use of. In the next paragraph he endeavours to persuade us, that electricity does not act upon the air ; tho’, three lines before, he says, that he knows that the electric æther is
attracted

attracted at very great distances in *vacuo*. Tho' many experiments do prove that electricity does act upon the air, I shall refer him to Mr. *William Watson's* experiments in *vacuo*, in the 47th vol. of *Philos. Trans.* page 362, where he has taken a great deal of pains to shew that air does act upon electricity, in confining it to non-electric bodies. If this be true, I think we may boldly affirm that electricity re-acts upon the air. And I have not any reason to doubt Mr. *Watson's* doctrine (as far as it relates to the pressure of the air upon an electric atmosphere) but that he now recommends Mr. *Darwin's* doctrine, which is directly contrary; which may be from some new discoveries which I am not acquainted with.

To illustrate that air is not acted upon by electricity, Mr. *Darwin* produces an experiment with a long-necked bottle suspended by silk, with the neck downwards, immersed in oil of turpentine, supported by an electric; and the inside of the bottle lined with gilt paper; and then warmed so, that, when cold, the oil was forced up half way the neck. Then with a bent wire he electrified the inside of the bottle; and finding that the oil was not forced out of the bottle, he concludes that the electric atmosphere did not act upon the air. If this gentleman had considered the force necessary to move this column of oil, which force must be able to support a column of the same height, and a base equal to the whole surface of the inside of the bottle; and
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that air is an elastic body, and that the electric atmosphere is exceedingly so, perhaps his surprize may lessen. And if he will please to add to this what I have said in regard to charging the *Leyden* bottle, that as one power of electricity goes into the bottle, an equal quantity of the other power comes out; and to consider how minute a part of that power, which predominates in the bottle, is expanded into an atmosphere, he will scarcely think the force of this atmosphere equal to the force which I have said was necessary to drive the oil out of the bottle. Add to all this, that this atmosphere has a power of passing thro' the glass; which he may easily try, by touching the outside of the bottle with a piece of bog-down suspended by silk, and he will find the outside of the bottle as much electrified as the inside, and with the same power of electricity. I could now produce an experiment which would nearly ascertain how much an electric atmosphere presses upon the air in such a bottle; but that I fear I may tire your patience.

In the next paragraph Mr. *Darwin* tells us, that he had formerly tried this experiment various ways, in order to make the electric matter answer the end of steam in the steam-engine, and many other great mechanical purposes. In this I think he has given us a full proof of a strong inventive faculty, and a clear unbiassed judgment (which, in the beginning of his letter, he says, are necessary for making experiments) for I dare say that he did not
steal

steal this design from any man living; to apply a power to great mechanical purposes, without knowing any thing of the force which that power acted with, or its manner of acting. And yet I can tell Mr. *Darwin*, that the business of the steam-engine is mostly carried on by the electric powers; but shall not now stop to explain this matter. If Mr. *Darwin* has a mind to enquire how these subtile parts of matter act on each other, I would desire him to enquire how it comes to pass, that in a room filled with air, the rays of light can cross each other in so many millions of angles, as is necessary to give the eye a power of seeing any part of the room, to which a right line can be drawn, let the eye be moved as often as you please from its first station; next, how all the operations of electricity can be carried on in the same room; also, all the operations of magnetism; the operations of culinary heat, and all the effects of gravity, by which every particle of the whole is affected; and yet these powers give but little obstruction to each other, which can be made evident to our senses. When Mr. *Darwin* has explained these things, it may help me to explain his experiment.

Mr. *Darwin* now proceeds to shew you the fallacy of my experiments; but, unluckily, as far as he has gone, he has proved the truth of them; viz. that ascending vapour does pass by electrified bodies, without divesting them of that power which electrified them. But why they do so, is not to be understood

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without a perfect knowledge of the action of the different powers of electricity. Indeed he has shewn a thorough ignorance in making electrical experiments, and substituted a monstrous falsity of his own. First, he supposes that the *Leyden* bottle, a dried feather, a dried cork, and the bog-down, which I have mentioned, are equally fit to try electrical experiments with; whereas a dried feather and the bog-down are as different matters for this purpose, as glass and metal. He says, that the *Leyden* bottle may be touched three or four times by a quick finger, before the whole is discharged. While the bottle was suspended by silk, I have touched the top and bottom alternately with my finger, seventeen hundred times before it was discharged; tho' there were but twelve cubic inches of water in the bottle. A dried feather will not conduct electricity more than dried silk, or glass, and therefore, like them, when electrified, may be touched in many parts, before all the power is withdrawn, by which it was electrified. A dried cork acts something in the same manner; but the bog-down conducts electricity nearly as quick as metal; and, therefore, when electrified, cannot be touched by the finger, in any part, for half a second before all signs of its being electrified disappear. A piece of this down, suspended by silk, against the side of a conductor, will play between the finger and conductor, while the wheel is turning, so quick, that the silk, by which it is suspended, will appear like a narrow

row ribbon; and it is evident, that while it moves to the conductor, it is electrified with the one power; and when it moves from the conductor, it is electrified with the other power of electricity. Yet has Mr. *Darwin* the assurance to tell the Royal Society, that this down may be touched at intervals, for half a minute, by the finger, or any known conductor of electricity, before it would part with its electricity. This experiment is so very easily made, that the Royal Society cannot long doubt which of us has told them an untruth. Mr. *Darwin's* words stand thus: "From whence I apprehend, that Mr. *Eeles*, "having dipped the electrified down of the "juncus bombicinus in vapour, for perhaps "half a minute (for no time is mentioned) "and finding it still retained its electric attraction, was not aware that this same had "happened, if he had by intervals touched "it with his finger, or any other known conductor of electricity."

In my letter, where I have mentioned this matter, my words stand thus: "But to try "whether the steams, &c. were non-electrics, "I only bedewed the wax and glass with my "breath, steams, &c. from my hand to the "end of the wax and glass; and then touching the electrified down with the end of the "wax or glass, I found that the electrical fire "immediately passed from the down into my "hand, through the steams, &c. which rested upon the wax and glass."

Now whether this strange apprehension of Mr. *Darwin's* (where he apprehends that all

did not do, what I expressly said I did do) and that the effects of an experiment (which I can't believe he ever made) would be contrary to what I have declared they were, and to what, I now assert, they always will be, is sufficient to shew the fallacy of my experiments, and to overturn all I have said about the cause of the ascent of vapour, &c. I must leave to the Royal Society to determine.

Next, Mr. *Darwin* takes a great deal of pains to prove what every body knew, and what I never contradicted; but, on the contrary, positively asserted; viz. that light bodies electrified by glass were attracted by electrified wax, and so *per contra*: what I said upon that occasion was expressly a hint to enquire, whether electricity was not differently actuated by different qualities in the wax and glass? which I have since found to be true; and also, that electricity itself consisted of two distinct different qualities or powers; which I have proved by numberless experiments, without ever meeting any one experiment to contradict it. From which, and many observations thereon, I have been enabled to apply the electric powers to medicinal use, from a reasoning *a priori*, which I think nobody else has yet attempted. And I have performed some cures, which the physicians here thought very little less than miraculous; and indeed I thought so too, the electrical powers having perform'd more than I could possibly expect in many cases. And to give you an idea of what they will do, when properly applied, I beg leave to mention what was said by a patient

tient of mine, who had been with me for some time.

Mr. *Thomas Gard*, who was universally paralytic, except his head, and who was bred an apothecary and chymist, has enquired of me, with surprize, in the following terms; viz. Dear Sir, how is it possible for you to tell me beforehand what you intend to do; and then, in a few minutes, to give my limbs a power of what motions you please; and that you can retract that power when you please, and give them a power of other motions as you think proper; that they seem no longer obedient to my will, but to yours.

I had drawn up an account of some of these cures to lay before the Royal Society, not so much from a vanity to tell what I had done, as to caution the unskilful not to attempt to use the electrical powers in a medicinal way; for great mischief may be done by them. I shall here only mention that three things are necessary to any attempt of this kind; which do not meet in every man who has an electrical apparatus. A skill in medicine, a skill in anatomy, and an accurate skill in electricity and the manner of conducting its powers.

In the next paragraph Mr. *Darwin* quite triumphs over me. "But what alone would entirely destroy this electric hypothesis is, that, from the experiments of Mr. *Franklin* and others, the clouds are sometimes found to be electrified plus, sometimes minus, and sometimes manifest no signs of electricity at all."

Altho' I do not know that any body besides Mr. *Canton* has taken notice that the clouds are frequently electrified by the different powers of electricity, which he calls positive and negative; yet I can easily grant that they are so, because I know it to be true from a great number of experiments which I have made. But, pray how does this entirely destroy my hypothesis? Does it not confirm it as much as is possible, as I have shewn in the course of this letter? His last assertion, that sometimes the clouds manifest no signs of electricity at all, may be true, as to any experiments which can be made by Mr. *Franklin*, or by Mr. any body else, on a light cloud, at a thousand or two thousand yards distance; and yet the particles of that cloud may be highly electrified, though their atmospheres are not pressed off to this distance.

Here Mr. *Darwin* has again been unlucky, and has proved the thing which he wanted to disprove.

And what is more, almost the only thing which was wanting to compleat my system: For had I known at the time of writing my letter, that different clouds were frequently electrified with the different powers of electricity; and had I known how these powers acted on each other, it would have saved me a great deal of thought and contrivance, to shew how the electrified vapour may again return to the earth; as any body may see, who does me the honour to read my letter with attention; and would easily have proved how the

the whole quantity of electricity, which ascended, did again return to the earth; which, at that time, was impossible for me to prove, as I began late in enquiries about electricity; for at the time of writing you an hypothesis, to shew the cause of thunder, I had never read any writer upon electricity; nor had I any other knowledge of it, than what was furnished by my own observations upon some experiments, which were exhibited for two nights successively as a shew in the year 1748; and in the year 1750, accidentally thinking that the cause of thunder was not sufficiently explained, I recollected that the electric fire subsisted in water, and finding the seat of thunder was also in water, I began to think how the electric fire may be the cause of thunder; and from thence drew up that hypothesis, which, tho' entirely my own from that slight assistance, I may say, is as full and clear (as far as electricity was then understood) as any thing that I have met with, which has been produced upon that occasion.

And as that discovery was as much my own invention, as if an analogy between thunder and electricity had never been thought of by any body else; so without any other assistance, than my own experiments and observations, I have pursued the explanation of the cause as near to a demonstration, as the nature of the proofs will admit. It is true that I have read some tracts upon electricity; but as they came late to

my hands, I can't say that they gave me any assistance in my enquiries; on the contrary, I must say, that if I had rested in any of the opinions which I met with, I must have stopped short of the truth.

The other part of Mr. *Darwin's* aforementioned paragraph ends so forcibly, and with so polite a compliment to me, that I cannot forbear troubling you with it.

'Whence to say an accumulation of electric æther supports these clouds, seems an assertion built upon a very unstable foundation, whose whole superstructure may well enough be termed an air-built castle, the baseless fabric of a vision.'

I must confess, gentlemen, that I have built my clouds upon the air, and every body sees that the foundation is sufficient; but Mr. *Darwin* has vastly outdone me, for he has built without any foundation at all. I am,

GENTLEMEN,

your most humble and

most obedient servant,

HEN. EYLES.

I have insensibly been drawn into a prolixity by answering the absurdity of Mr. *Darwin's* theory, and remarks upon what I had wrote; for which I am conscious that I ought to ask your pardon. My chief design was only to give a farther explanation of the cause of the ascent and descent of vapour and exhalation,

exhalation, &c. and to shew that they ascend and descend by the action of the same powers; and that the powers entire may both ascend and descend with them; which never can be explained by the doctrine of positive and negative electricity, or a plus and minus of the same power. So that my doctrine of the two powers of electricity must be accepted, or electricity be excluded from the business of the clouds; which cannot well be allowed against the evidence of so many well-known experiments.

* * Sent this letter, dated 25th March, 1761, to the Royal Society.

LETTER IX.

SIR, *Lismore, 26th Feb. 1760.*

SINCE my letter of the 14th February, 1758, I have made many electrical experiments; all confirming the doctrine and laws which I mentioned in that letter. As some of them are quite inexplicable by any thing which was said of electricity before that time, or indeed by any thing I have since met with, I shall mention two or three of them. Let a conductor be electrified, either by a glass globe or a globe of sealing-wax: I can, in a few seconds of time, with the atmosphere of that conductor, electrify a large plate of glass, or a thin plate of sealing-wax; strongly on both sides with the vitreous pow-
er;

er; and then withdraw the vitreous power, and leave both sides electrified with the resinous power; or then can withdraw that power from one side, and leave the different sides electrified with the different powers; and all this without any contact with the conductor; and the plates shall remain so electrified, in any of the aforementioned states, when carried quite out of the atmosphere of the conductor: Or I can charge Mr. *Muschenbroek's* bottle, suspended by a dry silk cord in the atmosphere of that conductor, several inches from the conductor, and without any contact with that, or any other matter but the silk cord, first with the vitreous power inside, and the resinous power outside; and then withdraw that charge, and recharge the bottle with the powers in the contrary order; without the bottle's having any communication with any thing but the silk cord. I shall take another opportunity of explaining this experiment, and of shewing that the electricity found in the air during the time of thunder, &c. does not only come down from the clouds, but is also drawn up from the earth by the electric atmosphere of the clouds. I can, with an electrified glass, electrify a piece of down through a glass which is not electrified; and that with either of the electric powers, as any spectator shall please to direct at the instant of performance: Or I can do the same thing with an electrified stick of sealing-wax: Or, with the same piece of wax, I can electrify a piece of bog-down, suspended by

by silk, with either of the electric powers; and then withdraw that power, and electrify it with the contrary power; and so on for several times in succession; and yet a spectator shall think that I touch the down with the wax in the same manner to give both the powers.

I would now ask how these experiments are to be explained by the doctrine of positive and negative electricity? or that glass throws out the electricity, and that wax drinks it in? or the supposition that glass is impermeable by electricity? or the wanting and abounding sides of the glass in the charged bottle, as *Mr. Franklin* has explained it; that one side of the bottle parts with exactly the same quantity of electricity which the other receives; and, on forming a communication between the inside and outside, that the emptied side greedily drinks up the superfluity of the abounding side. But I will not detain you with arguments of this kind.

Thus you see I have called these powers vitreous and resinous, merely for distinction's sake; for I do not know of any different essential characteristic to distinguish them by. I had some regard to their being so called by gentlemen, who fancied that they found them existing singly, the one in glass, and the other in wax, &c. For *Mr. Muschenbroek* says, that nobody can be in possession of both powers at the same time: in which he is so far mistaken, that these powers can never be intirely separated

separated in any body, or brought to any thing like an intire separation.

I have ventured to try various experiments on the human body with these electrical powers; by which I have introduced them into medicinal use, and have performed many surprising cures in paralytic, rheumatic, and many other cases; and have done some things with them which were not thought to be in the power of the *materia medica*. An account of which I will lay before the Society, if they think it acceptable.

In my letter of the 9th of *August*, 1757, I mentioned a great similarity between the action of the electric and magnetic powers; on more mature examination I find, that the laws which I have given, in my letter of the 14th of *February*, 1758, for the action of the electric powers on each other, will exactly serve for the action of the magnetic powers on each other: Though, to a person not well versed in these matters, there may appear a seeming difference, which, upon examination, will be found to arise merely from the different position in which the different powers in the magnet are placed, from that in which the different powers of electricity are placed; for in electricity the powers change place from one body to another, which in magnetism they do not. And this may prevent the troubling you with the recital of those laws; or if you think fit to be troubled with it, I can recite those laws in a clearer
and

and more connected manner than I have yet done.

Herewith I send you a letter to the Royal Society, shewing what I think is the cause of the reflection and refraction of light.

It is impossible for me to foresee all the objections that will be made to it; or if I were to answer beforehand all the objections which I think will be made to it, I must enter upon a long tract. But as Sir *Isaac Newton* is the great master in the doctrine of light, I think it will be sufficient to shew, that the causes, which he has pointed at, are not sufficient for the purpose. He has mentioned air; the thickness of the transparent bodies; the action of bodies upon light, at a distance; (by which, I suppose, he means an attraction) and his subtile medium, evenly diffused over the surfaces of bodies.

As to air, he has shewn that it is not the cause; and, if he had not, there is a self-evident reason against it; because refraction is performed between the confines of air and all other bodies. The thickness of transparent plates cannot, in itself, be the cause; for he has shewn that reflected and refracted light does not impinge on bodies; indeed the thicker transparent plates are, the more the refractive power will adhere to them. As to the action of bodies upon light at a distance, (which Sir *Isaac* seems to stick to; for, in his fifth query, at the end of his book of optics, he asks, do not bodies act upon light, in reflecting, refracting, and inflecting

of it). I think it cannot be the cause; for when a ray of light has passed through a transparent body, if that ray be again drawn back into that body by attraction, that attraction must be strong enough to hold that ray affixed to that body, as long as the attractive power remains the same; and therefore there never could be any reflection back again through a transparent body; but all experience shews such a reflection to be common. As to a subtile medium, evenly diffused over the surfaces of bodies, it certainly must be the cause; and had he rested it there, he would have been very near the truth. But when he had a mind to apply it to other purposes, and to make it the cause of gravity, he proposed his æther of such a form as would not answer either of his purposes: for he supposes it to be a subtile elastic medium, with an increment of density from the sun through all the planetary regions, pressing the planets from the denser into the rarer parts of this medium; and thus acting as a centripetal power to keep them in their orbits. But I must here observe that this æther, as a centripetal power, could only act on the planet with a force proportional to the increment of its density, in the length of the diameter of the planet; but it must obstruct the progressive motion of the planet with a force equal to its whole increment of density, from the sun to the center of the planet, and in these circumstances can never be the cause of gravity; and for many more reasons which

I could

I could add ; one of which is, that gravity is performed by one single power, which all bodies, and all parts of bodies, have of attracting each other ; which can never arise from such an æther. As to such an æther's being the cause of reflection and refraction, &c. how can we suppose it ? for if the reflecting medium can turn back the rays of light in the 89,000th part of an inch, as Sir *Isaac* says it does, how is it possible for light ever to arrive to us from the sun through such a medium ?

I desire that what I have said here may be looked on as a sincere search after truth, and not as a reflection on the character of so great a man as Sir *Isaac* certainly was ; for no man has a higher esteem for his memory than I have ; and this matter he himself has left and recommended to our enquiry ; and I must observe, that it was impossible for him, or any man at that time, to know what a subtle medium, and how fitted for the purpose of refraction, &c. did really supervade the surfaces of all bodies. Nor is it now easy to conceive, but by those intimately acquainted with the doctrine which I have given of the electric powers ; and by such as have gone through a great number of experiments, to convince themselves of the truth of that doctrine. Sir *Isaac Newton*, in speaking of his secondary attraction and repulsion at very minute distances, makes a great guess at the use of electricity in that case ; for he expressly says, who knows but the power of electricity may extend itself to these minute distances without

without being excited by friction, which is certainly the case. And here that attraction and repulsion may be easily shewn to be the effect of the electric powers: in the attraction the force of gravity must also act. But I believe I had better explain this matter in a paper by itself. Had he then known as much of electricity as we now do, and that the electric powers adhered to all bodies, I am certain that he would have made a better use of that knowledge than I am able to do. However, I am confident that in time the electric powers will be fully proved to be the cause of the reflection and refraction of light.

I shall be very much obliged to you if you will let me know the most material objections which shall be made to what I write to the Society. I am,

S I R,

Your most humble and

most obedient servant,

HENRY BELES.

To the Rev. *Thos. Birch*,

Secretary to the Royal

Society.

L E T.

L E T T E R X.

Lismore, 26th February, 1760.

GENTLEMEN,

IN the year 1752, I promised you to shew that the fire or fluid of electricity was the cause of the refraction, &c. of light; and accordingly wrote down my reasons, which, on farther consideration, did not appear satisfactory to me, and therefore did not trouble you with them; nor was it possible then to explain this matter, the action of the electric powers not being at all understood. But since I have discovered that all the phænomena of electricity arise from the action of two different distinct powers, which always act conjointly, and explained their manner of acting on each other; (which I communicated to Doctor *Birch* in two letters, of the 9th of *August*, 1757, and 14th of *February*, 1758) it appears plainly to me, that these powers are the cause of the reflection and refraction of light.

The great difficulty of explaining refraction was, to shew why the rays inclined to the perpendicular, in passing from a rare into a dense medium; and receded from the perpendicular, in passing from the dense medium into a rarer one. I will not detain you to shew the insufficiency of all the reasons assigned,

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signed,

signed, but must beg leave to object to the principal one ; which was, supposing that the refracting medium had a vibrating motion which accelerated the rays in their passage through it ; which, I think, must be wrong.

For first, how is it possible that any body can be accelerated by passing through any medium ? Must not the body meet a resistance in proportion to the depth and density of the medium ? If the medium moves faster than the body, how can the body overtake the medium to pass through it ? Is it intelligible how any body in passing through any medium, can put that medium into such a motion as may accelerate the body ? If it be supposed that the medium is naturally in a vibrating motion, to make the pulses quicker than the motion of the body passing through it, its velocity must sometimes be as great against the motion of the body, as at other times it is with it ; and when reflection is caused by such a vibrating motion, the angle of reflection must differ as much from the angle of incidence, as the angle of refraction does ; the rays being as much accelerated in reflection, as in refraction, by such a medium. Must not these vibrations incline the rays as much to the perpendicular in their passage out of a dense body into a rare one, as they do when the rays pass out of a rare body into a dense one ? But this is contrary to all experience ; and it is more difficult to explain
how

how such a vibrating motion, the pulses of which shall have a greater velocity than light, can be caused and continued, than to explain all the difficulties of reflection and refraction, by means of a subtile medium at rest. Now if it can be proved that reflection and refraction may be caused by a dense elastic medium of subtile parts, and that such a medium does really exist, covering the surfaces of all bodies, and that in proportion to the density of the bodies; (for Sir *Isaac Newton* has shewn that bodies have a refractive power in proportion to their densities) I think that the doctrine of reflection and refraction (for as for deflection or inflection, it will appear to be only refraction by the different position of the medium) will be made much more intelligible than it has been hitherto, and the cause of reflection and refraction fully proved. Sir *Isaac Newton* has shewn that sulphureous bodies have a greater refractive power than other bodies, in proportion to their densities; it will be here also shewn, that this medium adheres to such bodies in greater quantity than to other bodies of the same density.

The above great author, in his first book of optics, has given us a clear doctrine of the refrangibility of light, and of the separation of its colours thereby; and an intire confutation of that doctrine which supposes light to be propagated by pression; so that I need not take notice of what has been said by some

gentlemen who have lately attempted to revive that doctrine. But as the cause of refraction, &c. was confessedly unknown to this great man, he only supposed that it must be performed by the pulses of some subtile medium, which press forward the rays in refraction.

As to the manner of acting, I have before objected; and I shall now endeavour to shew how refraction may be performed by a subtile medium's retarding the rays of light in their passage through it; and to shew what that medium is, and the manner how it acts.

I shall first consider the case of a ball shot through a board. Suppose the line of direction of the ball to make an angle of forty degrees with the plane of the board, and another plane inclined to the plane of the board in that direction, dividing the ball into hemispheres; it is plain that the hemispheres next to the board must first come in contact with it, and give the ball a direction from the perpendicular to the board, till the ball is intirely immersed; and whatever direction the ball then has, it must retain till it begins to immerge, and then the direction must be altered in the contrary order from which it is immersed, and proceed in a line parallel to the line of its first direction.

I will now consider this board of another contexture, with the side very rare which is to be entered by the ball, and that side very dense from which the ball is to immerge; the
consequence

consequence will be, that the ball shall not be so much altered in its direction from the perpendicular in immersion, as it shall be altered to the perpendicular in its immersion from the dense side of the board; and therefore the angle of refraction will bear a similar proportion to the angle of incidence, as it does in light passing from a rarer into a denser medium. And now suppose the ball passing back again through this board, the angles of incidence and refraction shall be similar to those of light passing from a denser medium into a rarer.

Now I think all that is to be done to explain the refraction of light, is to find a subtle medium that supervades the surfaces of all bodies; whose denser side shall always adhere to the confines of the denser medium or body to which it is applied, and its rarer side always joined to the confines of the rarer medium or body.

Whoever attentively considers the doctrine and laws of electricity which I have given in my letters of the 9th of *August*, 1757, and 14th *February*, 1758, must see that the electric powers must always adhere to all bodies in this manner; for whether the vitreous or resinous power of electricity be encreased, the encreased power will act outward from the denser body, with an atmosphere with an increment of rarity outward, which will increase in proportion to the increase of the quantity of that power above the contrary lessened

power; which lessened power will always act inward to the denser body, in a condensed state, exceedingly more dense than the outward power. The truth of which doctrine I have evinced by a thousand experiments more than what I communicated in those letters; nor could I ever since produce any phænomenon, from any experiment which I could think of making, which was not easily applicable by the laws of electricity which I laid down in those letters.

Now considering the extreme subtilty and elasticity of the electric powers, and how exceedingly they condense each other when in or near an equal quantity with each other, which is their natural state in all bodies; the prodigious encrease of elasticity in proportion to their condensation; their immeasurable velocity in action; their adhesion to all bodies in proportion to the specific gravity of the bodies, except their being found in a greater proportional quantity on sulphureous and unctious bodies, which bodies have also a greater refractive power than other bodies of the same specific gravity; their attending all vapour and exhalations through the atmosphere, where the reflective and refractive powers are also found; witness the rainbow, crowns of colours, and all the coloured phænomena of the clouds: all these things duly considered, I think it no absurd conclusion to say, that these electric powers may be

be and are the cause of the reflection and refraction of light.

I could write much more fully on this subject, and add some things perhaps more explanatory of my meaning; but that I understand that the Royal Society do not chuse to attend long tracts; and I think that I could say something explanatory of Sir *Isaac*'s fits of easy reflection and easy transmission, but I shall not now trouble you with it. As to Sir *Isaac*'s furnishing the rays of light with different essential properties on their different sides, only to explain that single phænomenon of an unusual refraction in Island Chrystal; I must say, with the greatest deference to so great a man, that I do not think it so truly philosophical; for were the rays furnished with such different essential properties, all reflection and refraction must be confused, and the excellent use of vision rendered almost useless. I think it would have been better to have searched for the cause in the different positions of the refractive powers in the laminae of that fissil stone, where I think it may be found. x

And now, gentlemen, I fear that I have followed brevity too closely in the explanation of a cause so long sought after, a cause without which our noblest sense would be lost; and if it were possible for us to subsist without sight, our being must be more wretched than that of a mole. But if any of the gentlemen of the Society will be so very kind

as to let me know their objections, it will give me an opportunity of explaining this matter better, or of submitting and confessing that some other cause must be enquired for. I am,

Your most humble and

most obedient servant,

HENRY EELES.

To the Royal Society.

LETTER XI.

MY LORD, *Lismore, 29th Dec. 1761.*

WHILE you preside over the Royal Society, I hope that you will forgive my troubling you with a letter relative to the transactions of that Society. I am conscious that it must appear the highest presumption in any single man to contest with the Royal Society; and if I thought my present expostulation was with the whole Society as a body, I should have modesty enough to think so too, and desist from giving your lordship the trouble of this letter; but as the matter of my present complaint seems to arise from the unwillingness of a few members of the Society to understand some truths which I have produced to the Society, and that those truths are of the highest importance toward the making new discoveries, and the increase
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of natural knowledge, for which end, I presume, the Royal Society was peculiarly instituted, I shall venture to proceed.

But first I must beg leave to mention the tracts which I have laid before the Royal Society, and the success they met with; to shew the reason why I troubled them in particular, or the world in general, with any opinions of mine; and how I have been drawn into this expostulation.

In the year 1750, as I was accidentally reflecting that the cause of thunder was not sufficiently explained, I recollected, that in some electrical experiments which I had seen in the year 1747, that the electrical fire subsisted in water; and knowing that the seat of thunder was in heaviest showers of rain; and imagining that there was some analogy between the effects of thunder and those of electricity, I began to think how the electric fire may be the cause of thunder; and in *September*, 1751, I drew up an hypothesis to explain that matter; and, in *June*, 1752, I sent it to the Royal Society; and it was honoured with their acceptance, and printed in the *Philos. Transf.* for that year. At the end of that hypothesis, I mentioned some subjects which I thought might be explained by electricity; and the Royal Society did me the honour to desire my thoughts on those subjects. Accordingly, on the 25th of *November*, 1754, I sent them a letter, to shew the cause of the ascent of vapours and exhalations;

tions; and also to shew the cause of winds, both regular and erratic; and to explain the general phænomena of the weather, barometer, &c. which letter was also honoured with their acceptance, and printed in the *Philos. Transf.* for the year 1755. In this letter I mentioned some experiments, which the Society also desired me to send to them; and they are also printed in the *Philos. Transf.* with my letter. The great honour which the Royal Society had hitherto done me, drew my attention closer to these matters, and made me think it necessary for me to enquire farther into the nature and properties of electricity; and, if possible, to discover the general laws of its action, before I proceeded on the other subjects which I had mentioned; that I may be enabled to write more intelligibly to the Society, and to myself, than I found it was possible to do from what was then known about electricity.

At this time I perceived that all electrified bodies exerted two different powers, an attractive and a repulsive power; and, accordingly, have said in my letter, that when the particles of vapour were forced within their repulsive powers, they then ran into contact and spericity by their attractive powers; but, as yet, I knew nothing of these powers, or how they acted upon each other, or on the light parts of matter, by which they render their actions sensible to us. I therefore set myself to work upon electrical experiments, and,

and, after having made a very great number, I found that electricity consisted of two different distinct subtle elastic mediums, which were equally and strongly attracted to all matter, and that in proportion to the quantity of matter; and that these mediums strongly attracted and greatly condensed each other; so that when they were equal in quantity to each other, they were condensed into an exceeding minute compass; but when one was lessened, and the other increased in any body, the increased medium or power would, by its own elastic force, diffuse itself through a space many millions of times greater than that which it occupied in its condensed state; and yet may be instantly retracted to the body from which it issued, by superinducing on that body a proper quantity of the other power; that these electric atmospheres repelled any atmosphere arising from the same power, but attracted any atmosphere arising from the different medium or power: so that all the known phenomena, arising from electricity, are caused by these mediums or powers being equally attracted by all matter, their strong and equal attraction and condensation of each other, and their great elastic force; for a more particular explanation of which, I must beg leave to refer your lordship to the letters which I am going to mention.

When I had gone thus far in my enquiry, I wrote a letter to the Royal Society on the 9th of August, 1757, to shew the existence of these different

ferent powers in electricity, and to explain their manner of acting; and also to shew that magnetism consists of like powers, which act by the same laws, tho' I do not by any means think them the same thing. As I did not hear what reception this letter met with, I thought I had not explained my meaning sufficiently to be understood; and therefore, on the 14th of *February*, 1758, I wrote another letter, to explain this matter more fully by experiments, and to give general laws by which the electric powers act. I was something surprized that I never heard any thing of these letters, because I am as well assured of the truth of them, from numberless experiments, that two different distinct elastic mediums do exist in electricity, as I am that I live, or move, or have any being; and as I knew the subject to be of the greatest importance, as the electric powers are the great principle of motion in the world; and that their actions can never be explained, without allowing these two different mediums. However, on the 26th of *March*, 1760, I wrote to the Royal Society, endeavouring to shew that the electric powers are the cause of the reflection and refraction of light; and also to Doctor *Birch*, with some electrical experiments, which I think cannot be explained by any thing that has been said of electricity, without allowing the doctrine of two mediums which I have given; nor was it possible to conceive what I said about the reflection and refraction of light, without a perfect

fect conception of the action of the different powers of electricity. These letters also passed unnoticed; I suppose for the reason I have now mentioned.

About the beginning of the year 1761, I received the vol. of *Philos. Transf.* for the year 1757. This book came thus late to my hands, by being obliged to send to *London* for it; for our booksellers in this kingdom could not furnish me with it, tho' I often applied for it.

In this volume I met with three letters, signed *Erasmus Darwin*, Physician at *Litchfield, Staffordsbire*; which he is pleased to call a confutation of what I wrote about the cause of the ascent of vapours and exhalations, &c. His first letter is addressed to Mr. *William Watson*, with whom, he says, he is not personally acquainted, desiring Mr. *Watson* to lay his papers before the Royal Society, if Mr. *Watson* thought that they contained truth; but if he should deem them trifling, or futile, to suppress them.

His second letter, addressed to the Royal Society, contains a theory of his own to explain the ascent of vapours, which, he says, is founded on principles better known than mine; but, for my part, I cannot see any principles which he makes use of, but what contradict the reason and experience of all mankind (if such can be called principles) except that, he says, that heat makes vapours ascend; which, I think, every old woman knew before. His third letter, he says, is de-
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signed entirely to destroy my theory, and nearly to demonstrate, that the electric æther is far from having any share in causing vapour to ascend : how well he has done this his letter will shew. I shall only observe that his chief principle is, that the air is not acted upon by electricity. Now this is a flat contradiction of a truth that Mr. *Watson* himself has, with a great deal of pains and labour, established, and, I think, fully proved, by his experiments in *vacuo*, made for that very purpose ; to shew that electricity is confined to non-electric bodies by the air. Philosoph. Transf. vol. 47, page 362, &c. And tho' I know many experiments which prove the same thing, I choose to let Mr. *Watson* see that he has recommended a doctrine entirely opposite to his own principles, and shall leave him to explain the reason.

Mr. *Darwin* has attempted to shew the fallacy of some experiments which I sent the Society ; but he has only proved the truth of them, and substituted a monstrous fallacy of his own, and shewn that he is thoroughly ignorant in making electrical experiments ; but as I have given such an answer to Mr. *Darwin's* letter, as I thought it deserved, in my letter to the Royal Society, dated the 20th of *March*, 1761, I shall beg leave to refer your lordship to that letter ; which was chiefly designed to give a farther explanation of the cause of thunder, and the ascent and descent of vapours, from the discovery which I made
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that electricity consists of two different mediums or powers, and their manner of acting on each other; which could not possibly be explained without this doctrine.

For when I wrote my letter to explain the cause of the ascent of vapour, &c. I was conscious of one defect in it; that is, I found it impossible, from what was then known of electricity, to shew how the whole quantity of electric fire, which ascended and buoyed up the vapour, was ever to return to the earth again; and I knew that without such a circulation, that this great operation of nature could never have been carried on in the same constant manner, as it has been done from the beginning of the world: but finding the rest of my system very conformable to the phænomena of nature, and hoping that a farther research into the nature and properties of electricity may some way or other explain this matter, I let my letter go with that known defect. But since I have discovered the great elastic force of the electric mediums, by which either of them can (when freed from the attraction and condensing force of the different power) diffuse itself through a space of more than a million of millions of times greater than what it occupied in its condensed state, and yet be retracted and condensed to its former state, by an addition of the different medium or power; it is easy to shew how electrified vapour may be carried up by the expansion of the electric medium, and remain there till it
meets

meets with other vapours electrified with the different medium of electricity; in which case they must attract each other, and run into contact to form larger drops; and the electric mediums being then rendered nearly equal to each other in these drops, by an exchange of the different powers with each other in each drop, their atmospheres must be condensed into almost an insensible compass, and therefore cannot longer serve to buoy up the vapours, which must then fall in rain.

And thus the whole quantity of both the electric mediums which, in their expanded state, buoyed up the vapours, may, in a condensed state, return with them to the earth.

Now, that this is the real state of the clouds; that is, that clouds in near approach to each other are frequently electrified with the different mediums or powers of electricity, I have proved by a great number of experiments on the clouds; and have always found, that when two such clouds have drawn each other into contact, so as to let fall a heavy shower from the middle, that one end of such cloud was electrified with one power, and the other end of the cloud was electrified with the different power of electricity, while the middle of the cloud or shower shewed little or no sign of being electrified. Mr. *Canton* of the Society has also often found the clouds electrified with the different powers, which he calls positively and negatively electrified. This, my lord, would be sufficient

oient to prove the truth of what I have laid before the Royal Society; but since my writing the last mentioned letter, I have gone much farther to evince the truth of it; for I have fitted up an apparatus, by which I can make an artificial shower of rain with the electrical powers. For by electrifying two fountains of water with the different powers, they shall be dissipated into very small particles in their ascent, which particles shall attract each other above, and run into larger drops, and come down in the exact form of rain from the clouds. This, I think, comes as near to a demonstration as the nature of the proofs will admit. The only difficulty remaining is to shew how different clouds come to be electrified with the different powers; but the want of this knowledge does not any way detract from the truth of what I have advanced, since the clouds are really found to be so electrified.

But, my lord, this doctrine of electricity is not only useful to explain this important phenomenon of nature, but very many others which have puzzled the philosophers from the first ages; and modern philosophers cannot be ignorant, with what ardent zeal such active principles of attraction and repulsion were sought for, but left unknown, by the great *Newton's* self; yet not so far unknown, but that, in one great guess, he has expressly named the thing; tho' at that time he had not any possible means of proving the truth of his

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conjecture.

conjecture. For in his 31st Query, he says,
 'The attractions of gravity, magnetism, and
 'electricity, reach to very sensible distances,
 'and so have been observed by vulgar eyes;
 'and there may be others which reach to so
 'small distances as hitherto to escape obser-
 'vation; and perhaps electrical attraction
 'may reach to such small distances, even with-
 'out being excited by friction.' Which I now
 think it certainly does; and as I find the elec-
 tric mediums or powers universally adhesive
 to all matter, and that they can both attract
 and repel from very minute to very great dis-
 tances, they are fitted to answer many more
 of his purposes than could be then imagined,
 as they were supposed to reside but in a few
 particular bodies, and their manner of act-
 ing was quite unknown. I beg leave to add
 a few more lines from the latter end of this
 query, where he says, 'To tell us that every
 'species of things is endowed with an occult
 'specific quality, by which it acts and produ-
 'ces manifest effects, is to tell us nothing:
 'but to derive two or three general principles
 'of motion from phaenomena, and afterwards
 'to tell us how the properties and actions of
 'all corporeal things follow from those mani-
 'fest principles, would be a very great step
 'in philosophy, tho' the causes of those prin-
 'ciples were not yet discovered; and, there-
 'fore, I scruple not to propose the principles
 'of motion above-mentioned, they being of
 very

‘ very general extent, and leave their causes to be found out.’

This whole query, which consists of many pages, was designed by that great man to shew the necessity of the existence of principles of motion, many of which may not as yet be discovered, and to recommend a farther research into the nature and properties and laws of action of these we do know; three of which he has particularly mentioned; viz. gravity, magnetism, and electricity. Gravity, tho’ it may be the cause of compound motions in bodies put in motion by some other powers, I think is rather a principle of rest, as it inclines all bodies to move to each other in right lines, with a force proportioned to their quantity of matter and their distance; which motion once finished, gravity cannot again put them in motion. The other two seem to me to be more properly principles of motion, as they can both attract and repel, and are therefore fitted to preserve the motions necessary to vegetation, animal life, &c. Magnetism, as it is found in no other matter but iron, cannot be of very general use; however, it may be of much more use than we can at present know of. Electricity, as it is universally adhesive to all matter, and of great power, must be of very general use in the operations of nature; many of which I can point out.

Now the papers which I have laid before the Royal Society, and which have passed

unnoticed, go a great way in explaining the nature and properties of the two last mentioned principles of motion, and in giving general laws, which will explain all the known phænomena which arise from magnetism and electricity. I would therefore beg your lordship, that these papers may be carefully re-examined, and printed in the Philosophical Transactions; for as they are true, they must one day be found to be so, and will save others some part of the labour and pains which it has cost me for some years past to come at those truths.

But if any gentlemen of the Society think those papers unintelligible, or deficient in explaining the truths which they contain, I shall be greatly obliged to them if they will furnish me with their objections or observations, by letter, and I will return the most satisfactory answer in my power; and I believe I may set the matter in a better light now, than when I wrote; for I have had great experience in electricity since I wrote those letters.

I know I have mentioned many experiments, in my papers, which may appear very improbable, if not impossible, to many gentlemen who have made electrical experiments; but I solemnly assure you, that I have not mentioned one which I have not repeated an hundred times; nor did I mention them with any other view than that it may be known, when others come to make the

like

like experiments, that I was not ignorant of such matters when I wrote those papers.

But, my lord, I have since performed much more surprizing experiments on the human body, merely by the electric powers, in curing many paralytic and rheumatic cases, the cure of agues, chlorosis, and other disorders. And some of those cures were of so extraordinary a nature, and out of the power of the known *materia medica*, that our physicians here looked upon them as little less than miraculous; and in all these cures I have been enabled, from what I know of electricity, to proceed from a reasoning *a priori*, which I think nobody else has done in the application of electricity to medicinal use; at least I have not met with any account of it. For all the attempts to cure disorders by electricity, which are mentioned in the *Philos. Transac.* appear to me to be made without any previous rational design; and accordingly, they have done harm as well as good; and I believe that the accounts of mischief done would be more numerous, if all were as honest to confess it, as the gentlemen who have told of their disappointments; for I know that great mischief may be done by a wrong application of electricity in a medicinal way, and therefore gave a hint to the unskilful, not to attempt the use of it, in my last paper to the Society.

But I also know, that when it is rationally applied, that it may be the most powerful
medicine,

medicine, in many cases, that has ever been discovered; which I have sufficiently experienced for some years past.

I offered to give some accounts of this matter to the Royal Society; but as I did not hear from the Society, I supposed that they were not thought acceptable.

Though I fear that I have already tired your lordship's patience, I cannot forbear expressing my surprize at the unwillingness of some gentlemen to admit that electricity consists of two different distinct elastic mediums, since it may be demonstrated by almost every electrical experiment; and that there is not one single phænomenon, arising from electrical experiments, which can be fully explained without it. And it is the more extraordinary, as I find it has been thought all along that there were two kinds of electricity, but that they subsisted in different matters, as glass and resins, &c. and this as early as Mr. *Du Fay*; and Mr. *Muschenbroek* has expressly said, that there were two kinds, and that any body may be endued with either kind, but that nobody could be in possession of both together: in which this gentleman was so far mistaken, that it is impossible to separate them entirely in any body. Mr. *Benjamin Franklin* gives another doctrine, which has been followed, and says, that glass throws out the electric fire, and that resins drink it in; that is, that glass can superadd to any body a greater quantity of electric fire than its

its natural share, and that resins can take part of its natural share from any body; which he calls positive and negative electrifying; and supposes that bodies repel each other the more, the more they are divested of their natural share, in the same manner they do when there is an addition to their natural share. With deference to that ingenious gentleman, I can't forbear thinking it an odd way of reasoning, to think that bodies should repel each other the more, the more they are divested of the repelling power: but this is all a mistake; for glass neither throws it out, nor does wax drink it in; for in their natural state they are both endued with an equal share of both the powers; and these powers are brought into sensible action, by encreasing one power and lessening the other; and then the encreased power expands itself into an extensive atmosphere, which we call electricity; and either power may be encreased on the wax or on the glass; that is, I can make the glass cylinder act as wax commonly does; and when the wax cylinder acts as glass, and almost as strong as glass; in the sixth part of a minute I can make it act as wax again, and this with the same rubber; or, with a large piece of sealing-wax, with the same rubbing, I can electrify a piece of bog-down, suspended by silk, with either of the powers, and then withdraw that power with the same wax, and electrify it with the contrary power; and thus as often as I please;

and

and a spectator shall not perceive any difference in my manner of touching the down with the wax, to give either one power or the other, as he pleases to direct: now this is a full demonstration that both powers do exist without side the wax; or how is it possible for me to electrify with which ever power a spectator pleases to choose? These, and a great number of other experiments, which demonstrate, to the meanest capacity, the existence of these different powers in all bodies, I have shewn to hundreds of gentlemen, who can attest the truth of what I write, if required. And in my letter of the 14th of *February*, 1758, I have given experiments, which, if carefully examined, will sufficiently prove the doctrine of electricity which I have therein given: but if any gentleman is of the contrary opinion, I will submit to this trial: that if he can point out any one known phenomenon arising from electrical experiments, which I cannot fully explain by this doctrine; or if he can produce any one single phenomenon, which he can fully explain, without admitting of two powers in electricity; I will submit and confess my ignorance.

But I must beg that this disquisition may be carried on by letter; and these letters shall be faithfully laid before the Royal Society, if the gentleman please, that they may judge on whose side truth rests. I choose this epistolary correspondence, because my life is
too

too far advanced to wait for answers published in the *Philos. Transf.* which must come late, or perchance never reach my hands.

I believe that it is unnecessary to say any thing more on the importance of this enquiry, to ascertain general laws by which the actions of these general principles of motion may be explained; and therefore I shall conclude, in requesting your lordship, that I may have the honour of one line, to let me know that this letter gets to your hands. I am,

Your LORDSHIP'S,

Most humble and

most obedient servant,

HENRY EELES.

To the Right Honourable
George, earl of *Macclesfield*, President of
the Royal Society.

Q

•• My

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too far advanced to write for answers published in the Philistia, which will come late or perhaps never reach my hands. I believe that it is a necessity to say any thing more of the importance of this country, to ascertain general laws by which the actions of these general principles of motion may be explained, and therefore I shall conclude in repeating your lordship that I may have the honour of one line, to let me know that this letter goes to your hands. I am,

Your Lordship's

Most Obedient Servant



Henry Bunsen

To the Right Honourable
George Earl of Macclesfield
President of the Royal Society



My Printer informed me, during the printing of the foregoing work, that there would be a few pages blank at the close of those letters; I am therefore willing to fill them with an experiment or two, for the farther illustration of the reciprocal exchange of the electric powers.

FIX a wire to the under part of a coated bottle, so that the point may stand upright; and on that point place the needle with the reversed points. Place this bottle on an electric stand, with a communication from the conductor. All the time the bottle is charging, the needle will turn; but when the bottle is charged, the needle stops. Then touch the top of the bottle with your finger, or any conductor, and the needle will turn till the bottle is discharged. Now while the bottle is charging, if you touch the needle with a piece of bog-down suspended by silk, you'll find it electrified by the vitreous power, which flies off in exchange for the resinous power drawn in from the air to the outside of the bottle; and while the bottle is discharging, if you touch the down in the same manner to the needle, you'll find it electrified

with the resinous power, which flies off from the outside of the bottle in exchange for the vitreous power drawn in through the points from the air; while the vitreous power from the inside of the bottle makes the same exchange for the resinous power through your finger, to make these different powers equal to each other, within and without the bottle.

Place two *Leyden* bottles on an electric stand, with their coatings in contact; and while you charge one from the conductor, let a person on the floor touch the top of the other bottle with his finger; you'll find the first bottle charged with the vitreous power inside, and the second with the resinous power inside. Now the exchange here is evident; for while the resinous power from the inside of the first bottle changes place with the vitreous thrown in from the conductor, the vitreous, from the coating, changes place for so much of the resinous from the coating of the second bottle; and the vitreous in that bottle changes place for so much of the resinous power drawn in through the man on the floor.

I could easily furnish experiments sufficient to fill a great deal of paper; but I shall only mention one or two which I often made, and have shewn to many gentlemen versed in this science, which perhaps may puzzle the writers on this subject: but whoever can make them, must and will understand my doctrine of electricity; and plainly discover, that

that all that has been said about positive and negative electricity, or a plus and minus of the same power, has been to little purpose, and only served to keep men in the dark about this science.

I have a glass globe, intirely smooth, which, with the same rubber, I can make throw out the resinous or vitreous power, and electrify other matters with each power as often as I please; and generally I can charge these powers in one revolution of the globe; but in two or three revolutions I never fail to do it.

I have another glass globe intirely smooth, supported by one glass neck, from the equatorial line of which globe I can electrify with the resinous or vitreous power as I think proper, without applying a hand or any rubber to the globe, or any friction from the things to be electrified; and what is more, I can electrify two things with the different powers at the same time.

I hope my readers will forgive me, for not explaining these experiments, till I see who can make them, and explain them by the doctrine of positive and negative electricity, or a plus and minus of the same power.

As I have seen Mr. *Priestley* quoted, for improving and methodizing the theories of Mr. *Dufay*, and Mr. *Symmer*; I must observe, that Mr. *Dufay* never thought of the co-existence of these different powers in all bodies; nor even Mr. *Muschenbroeck*, long after him; therefore, there was not any theory

theory of Mr. *Dufay's* to be methodized. These papers will appear to have been wrote to the Royal Society, before Mr. *Symmer* thought any thing of the matter; however, as far as he has gone, he was right; and I think that Mr. *Priestley* has made too much of a trifling objection to what Mr. *Symmer* has said, that any man, from his own sensations, would be convinced that the powers came in opposition from both sides of the bottle, by feeling a slight shock in his wrists, a stronger in his elbows, and so on. Mr. *Symmer* did not say that these powers did not circulate through the body; but, that this sensation would shew their different direction: And, an explanation of these different sensations depends on a knowledge of anatomy, and the action of these powers on the different parts, which, it is possible, that both Mr. *Priestley* and Mr. *Symmer* may be strangers to. But this objection was only a pretext to mend Mr. *Symmer's* discoveries. Now, I believe it will plainly appear to any gentleman, who does me the honour to read these papers with attention, that the only theory which Mr. *Priestley* could have methodized, must be what he took from these papers; if an hypothesis can be said to methodize a doctrine which was clearly proved by experiments, ten years before: But I would ask which of Mr. *Priestley's* own numerous experiments, or what experiments of any other man, he has explained by his hypothesis?

or

or how he has supported this hypothesis, or whence he took it? For, he has not produced a single experiment in aid of it, to shew the existence, or manner of acting of these different powers: Or, I would ask whether his hypothesis has added any thing to the truth, or explanation of what I have wrote? or, if I had happened to have wrote that hypothesis, whether I should have put this matter in a clearer light than what I have done? or, what man would have depended on that hypothesis, without any other proof? So that I think Mr. *Priestley* may fairly own from whence he framed his hypothesis; for, after reading these papers, I believe that his hypothesis will not be found of any great use. Mr. *Priestley* has artfully shut me out of his history of electricity; though all these papers were addressed to the Royal Society, of which he is a member; and it appears from the first of them, that my attempt to shew the electric powers were the cause of thunder, was approved by that Society, and is the only attempt, of that kind, which stands recorded in the Philosophical Transactions. As for the rest of them, with Mr. *Priestley* and his hypothesis, I must leave to the judgment of unprejudiced readers.

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E R R A T A.

In the PREFACE.

- Page xxxiii, line last, *for its electricity, read*
its elasticity.
- xxxvii, l. 16, *for Mr. Gregg's, read*
Mr. Gray's time.
- xlvi, l. ¹⁶~~21~~, *instead of for, read*
from the then known
powers of nature.

In the LETTERS.

- Page 19, l. 16, *for united, read* emitted.
- 23, l. 28, *for incensing, read* meet-
ing.
- 37, l. 20, *for united, read* emitted.
- 90, l. 13, *for miles, read* turns.
- 93, l. 25, *for power to, read* power
on.
- 112, l. last but two, *for officers, read*
offices.
- 131, l. last but one, *for experiments,*
read experimenters.
- 165, l. 24, *for caminae, read* laminae.



